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THE

AMERICAN JOURNAL OF SCIENCE

[THIRD SERIES.]

ART. I.—*Researches in Acoustics;* by ALFRED M. MAYER.

Paper No. 9, containing:

1. The Law connecting the Pitch of a Sound with the duration of its Residual Sensation.
2. The Smallest Consonant Intervals among Simple Tones.
3. The Durations of the Residual Sonorous Sensations as deduced from the Smallest Consonant Intervals among Simple Tones.

1. On the Law connecting the Pitch of a Sound with the Duration of its Residual Sensation.

IN October, 1874, I published in this Journal "Paper No. 6 of Researches in Acoustics," which contained an account of my attempts to establish the law connecting the pitch of a sound with the duration of its residual sensation. The law given in that paper was the expression of the results of the first experiments, extending through several octaves, ever made on the duration of sonorous sensations.

Subsequently, in April, 1875, I published in this Journal the results of similar experiments which Madame Ema Seiler had made at my request. She made a long series of experiments with the same apparatus I had used. Her determinations, though agreeing with mine in having approximately the same variation of the residual sensation with the pitch, yet differed considerably in the absolute quantities which she found for the durations of these sensations. That the two

series of observations should differ was to be expected from the known variation of the sonorous sensations among different observers; but the principal cause of the difference is to be attributed to the apparatus, fig. 3, used in these experiments. This apparatus generated sounds in addition to the one to be specially observed, so that the determinations were difficult to make except by one whose hearing was peculiarly trained and naturally gifted in the power of excluding other sound sensations from the one alone to be studied. In the ability to analyze composite sounds Madame Seiler was noted, and I had no doubt at the time of the publication of her results that they were more worthy than mine to form the basis of a physiological law. This I stated in my paper of 1875, and the experiments described in the present paper, made with improved methods, show that the opinion then entertained was correct.

That there is a physiological law which gives the relation between the pitch of a sound and the duration of its residual sensation is shown by the numerous experiments contained in this paper. But those published in 1874 and 1875 sufficed to establish that fact; yet these experiments have never been repeated by physiologists.

I have waited nineteen years in the hope that others would make similar experiments, so that the combination of the results of various experimenters would give an expression of the law which might be regarded as general and accepted as expressing the average residual sensations of sounds.

It is true that Professor C. R. Cross and H. M. Goodwin published a series of similar experiments in "Some considerations regarding Helmholtz's Theory of Consonance." (Proc. Amer. Acad. Boston, June, 1891.) They obtained the smallest consonant intervals by blowing sheets of air across the mouths of resonators. The reciprocals of the differences of the frequency of the vibrations forming the intervals, thus found, are plotted in the curve, C, C, of fig. 1. I and I′ give their determinations of the durations of the residual sensations of UT_3 and UT_4, deduced from their observations of the coalescence of these sounds when interrupted by a perforated disk rotating between the resonator and its corresponding fork.

The curve, S, shows Madame Seiler's determination of the residual sonorous sensations; M, shows mine. It is evident that the meandering, undecided curve, C, cannot be the expression of a law, and that the data I and I′ cannot be combined with those contained in the curve, S, or, in the curve M. In a general way the curve, C, shows that the smallest consonant interval of two tones contracts as the pitch of the tones, forming the interval, rises.

The physicists Mr. Alexander J. Ellis and Professor J. A. Zahm have discussed the bearing of the law (as given by the experiments of 1874 and 1875), on the elucidation of many facts in consonance and dissonance, to which application of the law I referred in my paper of 1874.*

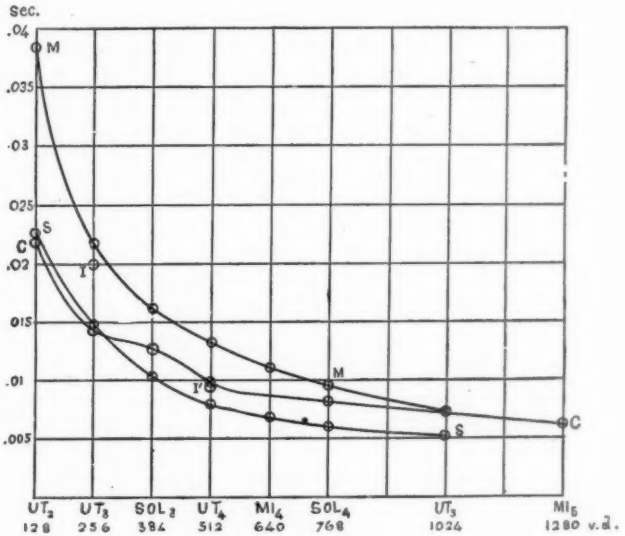

Fig. 1.

On the Duration of the Residual Sonorous Sensation.—The duration of a residual sonorous sensation is really the duration of the entire period in which a sensation of sound is perceived after the vibrations, outside the ear, giving rise to that sensation, have ceased to exist. While the total duration of the after-sensation produced by the stimulus of light can be measured (as in the case of an electric flash), the determination of the total duration or the after-sensation of a sound appears, in the light of our present knowledge and with the means of experiment at our command, to be a problem very difficult to solve.

The object of this research was not to determine the total duration of the after-sensation of a sound, but to measure that

*See Ellis' translation, of 1875, of Helmholtz's Lehre von den Tonempfindungen, pp. 173, 701, 795, and Ellis' Illustrations of Just and Tempered Intonation. Proc. Musical Assoc. of London, June 7, 1875. Zahm, Sound and Music; Chicago, 1892.

duration in which the after-sensation of a sound does not perceptibly diminish in intensity.

In fig. 2, D and E represent openings in a screen impervious to sound. The distance between these openings equals thrice the diameter of an opening. A tube, R, having the same interior diameter as the openings is supposed to convey

Fig. 2.

sound-vibrations against the screen, while the tube itself moves from left to right with its mouth sliding along the surface of the screen. In the position, A, the sound is just about to traverse the opening, D, to the ear on the other side of the screen. As R progresses over the opening, D, the sound traverses the opening till R has reached the position F B. Then, in the path of the tube from B to C no sound traverses the screen. When the edge B of the tube has reached the position C the sound is again just on the eve of traversing the screen through the opening E. As the distance A to B equals B to C, the periods during which the sound traverses the screen equal those in which it does not do so. If these alternations of sound and silence should succeed one another so rapidly that the sensation of the sound is uniform in its intensity; it may at first sight appear that during the time that the tube takes to go from B to C the after-sensation of the sound has not diminished in intensity. But is B to C to be taken as the measure of the duration of uniform sensation? As the tube, R, moves over D a sound with a varying intensity traverses the opening in the screen. We cannot suppose that the residual sensation caused by the stimulus of the sound traversing a minute opening in the screen equals that caused by the sound which traverses the screen when the circles R and D coincide. In such experiments, however, we are driven to take as the duration of the undiminished residual sensation the time that the center of the tube, R, takes to go from the center of D to the center of E.

In this illustration I have, for simplicity and conciseness, supposed the tube R to move over the openings D and E. In the actual experiments D and E are two of several holes in a disk, arranged in a circle, and the disk rotates while the tube R is fixed. Another tube placed in the prolongation of the tube R on the other side of the disk conveys the interrupted sound to the ear.

Evidently, the manner in which the tube conveying the sound to the disk is opened and closed by the revolving disk has to be considered in researches made with this apparatus. I give two cases whose discussion has led me to modify with marked efficiency the apparatus, shown in fig. 3, which was used in the researches published in this Journal in 1874 and 1875. In that apparatus the interruptions of sound were made by a perforated disk revolving in front of the mouth of a resonator while the interrupted sound was conveyed to the ear by a tube attached to the small opening in the nipple of the resonator.

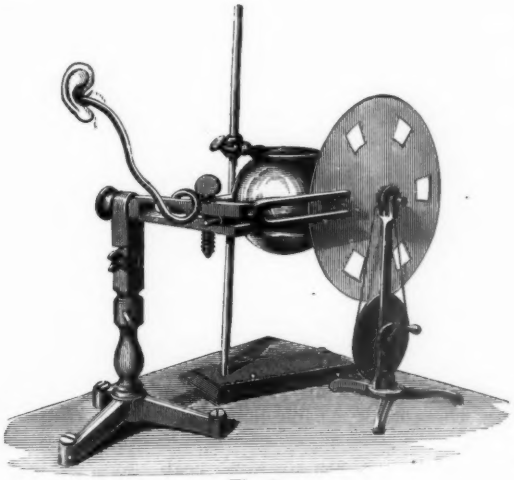

Fig. 3.

This mode of obtaining the interruptions of the sound is objectionable because the resonator is not in tune with the fork except when the former is fully opened and also because the perforated disk rotating across the mouth of the resonator gives rise to two secondary sounds and a resultant sound, fully described in my paper of 1875.* These sounds, from their intensity in this form of experiment, mask the proper sound of the fork, making the determination of the durations of the sonorous sensations both difficult and uncertain. Also, in these experiments the action of the interrupted sound on the ear is distressing; even injurious, for the hearing of one of my

*See a paper by Lord Rayleigh: Acoustical Observations, III, "Intermittent Sounds." Phil. Mag., April, 1880, in which the author gives an explanation, in mode and in measure, of the secondary sounds and of the resultant sound, observed by me in these experiments.

ears was permanently impaired by the experiments I made with this apparatus nineteen years ago.

In the apparatus, presently to be described, the fork vibrates in front of the mouth of the resonator and the interruptions in the flow of sound are caused by the perforated disk revolving in front of the small opening in the nipple of the resonator, as shown in fig. 7.

Discussion of the effects of the relative sizes of the openings in the revolving disk and of the opening in the tube conveying the sound to the disk.

First case.—Suppose that the opening of the nipple of the resonator and the openings in the disk have the same diameter. In the actual experiments these openings were 1^{cm} in diameter. The nipple of the resonator had a tube of that diameter adapted to it.

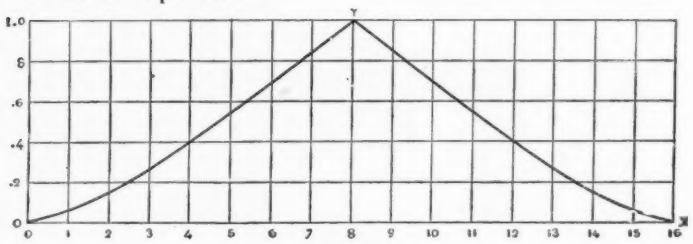

Fig. 4.

Fig. 4 is the graphic representation of the results of computing the varying areas of the opening of the tube of the resonator as an opening in the disk passes in front of it. This diagram is to be studied in connection with fig. 2. The entire length on the axis of abscissas, OX, gives the space A to B of fig. 2, divided into 16 parts. The ordinates of the curve give the relative areas of opening for corresponding positions on the axis of abscissas. The ordinate marked 8Y shows the full area of opening when the circles of the tube of the resonator and of the disk coincide. It will be observed that the tube is opened and closed slowly and is only instantaneously fully opened at 8.

Second Case.—The openings in the disk remain 1^{cm} in diameter, but the opening in the nipple of the resonator is $\frac{1}{2}^{cm}$ in diameter. Fig. 5 shows the relations between the areas of opening of the nipple of the resonator and the path A to B (fig. 6), of this opening, R, as it is supposed to move across the opening D in the disk. Fig. 5 is to be studied in connection with fig. 6. Fig. 5 shows that the opening and closing of the

nipple of the resonator takes place rapidly and that the nipple remains fully opened from 4 to 8; that is, during one-third the time that the opening in the disk takes to traverse the opening in the resonator. The advantages gained by this mode of experimenting are considerable. The periods of sound and of silence are sharply marked and, as we shall now show, the fact that the hole in the resonator has half the

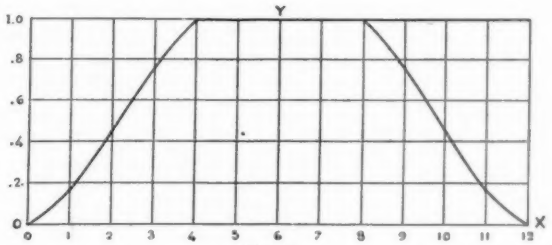

Fig. 5.

diameter of the hole in the disk gives us the means of approaching nearer to the measure of the veritable time during which we have an after-sensation of uniform intensity.

In fig. 6, R represents the opening in the nipple of the resonator, supposed to pass over the opening D in the disk. In this case, as in fig. 2, the space A to B in which sound traverses the revolving disk is equal to the space B to C in which silence supervenes, for the distance separating two holes on the disk equals twice the diameter of a hole, or, four times the diameter of the hole in the nipple of the resonator. But in this form of the experiment we are again in doubt as to what space should be taken as measuring the duration of the uniform residual sensation. The period from the full opening of the resonator in one position to the full opening in the succeeding one is equal to the time the disk takes to go from G to H, which is equal to the distance D to E between the center of the openings in the disk minus the diameter of the opening in the resonator, or, to DE minus $\frac{1}{8}$ DE. This distance, G to H, evidently measures the periods between maximum and maximum intensities of succeeding sound pulses, and we have taken this distance, in terms of velocity of rotation, as the measure of the period of uniform residual sound sensation because we have no certain knowledge of the relative durations of the residual sensations corresponding to

Fig. 6.

vibrations which pass the disk with increasing intensity, from 0 to 4, fig. 5, and with decreasing intensity from 8 to 12.

In our experiments we measured the number of flashes of sound entering the ear by knowing the number of revolutions of the disk per second and the number of holes in the disk. From this knowledge we compute the time it took the disk to go over D to E, in fig. 6, the distance between centers of two neighboring holes; then we reduced this time by $\frac{1}{6}$ which is the ratio of the diameter of the opening in the nipple of resonator to the distance, D to E, and took this reduced time as the duration of the uniform residual sensation. The duration of the sonorous sensation determined in this manner is evidently nearer the truth than that obtained with apparatus in which the hole in the tube conveying the sound to the disk and the holes in the disk have the same diameter.

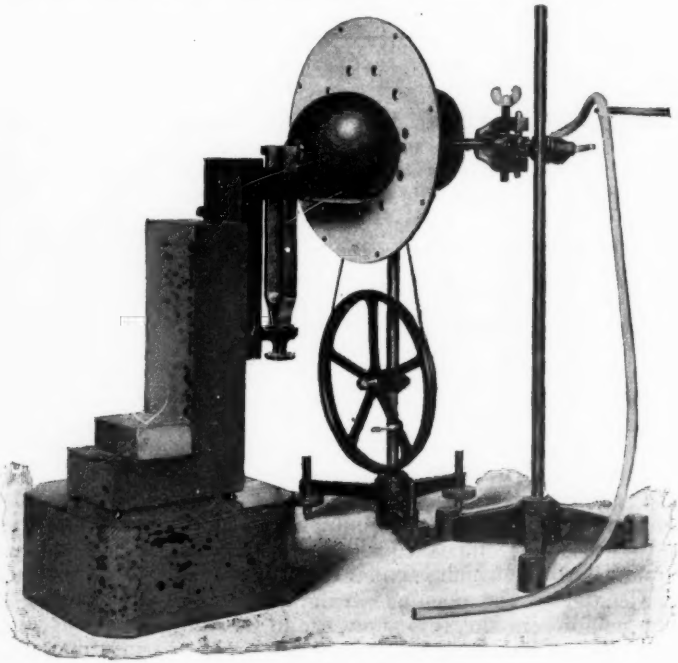

Fig. 7.

The Apparatus and Methods used to measure the durations of residual sonorous sensations.—(A) I shall first describe the apparatus which I found the most efficient for these measures

and then describe three other forms of apparatus used. In fig. 7 is seen a perforated disk mounted on the axle of a rotator. In front of the disk is the resonator with its nipple close to the surface of the disk. A tuning-fork is opposite the mouth of the resonator. On the other side of the disk, with its axis in line with the axis of the nipple of the resonator, is the tube which conveys the interrupted sounds to the ear. The opening in this tube has the same diameter as the openings in the revolving disk. Behind the line of this tube is seen, on the end of the axle, a fly wheel of copper weighing a kilogram.

The bevelled face of the driving wheel of the rotator has three grooves cut in it, in steps. Corresponding grooves are cut in the pulley of the axle.

To run a rotator of this description smoothly, without jars, or vibrations, it is necessary that the cord which passes over the driving wheel and pulley should be very flexible and have a circular section of uniform area throughout its length. I obtained such a cord by soaking a cord of porpoise leather in neat's foot oil and then drawing it many times through wire-drawing plates. The ends of the cord are connected by a short hook and eye coupling. The driving wheel can be slid up or down the standard of the rotator to adjust the tension of the cord.

The driving wheel is turned by a handle of aluminum of the form shown in the figure. It is necessary to have a handle of small diameter in order to turn the wheel with a uniform velocity. The fingers which clasped the handle were coated with plumbago dust.

In all the experiments the driving wheel of the rotator was revolved either once in a second or twice in a second. This is accomplished, after some practice, in the following way. The rotator, on which is mounted the disk and fly-wheel, is placed near a clock giving loud beats of seconds, and the driving wheel is revolved by the guidance of hand and ear The results of the experiments showed that the velocities thus given to the disk were sufficiently uniform, and the measures of the durations of sonorous sensations sufficiently concordant and precise to obtain the data of the physiological law.

I adopted this method of rotation in preference to mechanical means for controlling and measuring the revolutions of the disks. To determine when the interrupted sounds have blended, requires, so to say, a flexible apparatus whose velocity is under the immediate control of the hand and ear. This is important in making the final judgment between sounds, one of which appears to have too few interruptions, the other a few more interruptions than are necessary to give a continuous uni-

form sensation. It is evident that when we can at once slightly increase or diminish the velocity of rotation of the disk we have the means of making comparisons rapidly succeeding one another. A rotator driven as described forms more a part of the observer than one driven and regulated by mechanism.

As there were three grooves in the driving-wheel and three on the pulley on the axle, and as the driving-wheel was revolved either once or twice in a second, 18 different velocities could be given to the rotating disk.

Disks were made having numbers of holes from 5 to 19, so that, with 18 velocities and the various numbers of holes in the disks it was easy to select a disk driven with a known velocity which gave the exact number of interrupted sounds per second to blend.

The 18 ratios of velocities of the driving-wheel and of the pulley on the axle of the rotator were obtained as follows: A circle of card-board, divided into 100 parts. was clamped on the rotator in front of a disk. The driving-wheel was rotated either once or twice in a second so that the conditions were the same as in the experiments. From 10 to 100 revolutions of the driving-wheel were made before the ratio was determined. The division to which a fixed index pointed on the divided circle gave the fraction of a revolution. The whole number of revolutions was given by a simple counter which moved with very little friction.

The rotating disks were made of mahogany, 5^{mm} thick, with disks of card-board about 2^{mm} thick screwed to the wooden disks. The circumference of the holes in the wooden and card-board disks exactly coincided. The circle of holes in the disk was placed 5^{cms} from its border. The rotator and disks were so carefully made that the nipple of the resonator was only $\frac{3}{10}^{mm}$ from the surface of the revolving disk. The mouth of the tube conveying the interrupted sounds to the ear was about the same distance from the surface of the other side of the disk. The disks were clamped on the axis of the rotator between smaller flat disks of brass, not shown in the figure.

The diameter of the holes in the disk and of the interior of the tube conveying the sounds to the ear was 1^{cm}. The diameter of the openings in the nipples of the resonators was $\frac{1}{2}^{cm}$.

The disks were made of mahogany which had been in my possession for thirty years. It was well seasoned and had nearly the thickness required for the disks. This wood was used because it holds the form given it better than any wood I have had experience with.

Sound passes through mahogany and other woods even when a centimeter in thickness. Sound also passes through card-board but not so readily as through wood. I found that by placing card-board on wood I formed a screen of heterogeneous materials which presented an effective obstruction to the passage of sound.

(B) In the second form of apparatus I replaced the resonators by resonant tubes as shown in fig. 8, where F is the fork, T, the tube with a tube of larger diameter, A, sliding on T, so that the air in the tube could be adjusted to vibrate with the fork. On the other side of the disk D, is the tube T′ to which is attached a tube of caoutchouc which leads to the ear. This arrangement is like that used by Dr. R. Kœnig and described on page 140 of his "Quelques Expériences d'Acoustique," Paris, 1882, and used by him for the observation of the sounds produced by interruptions of continuous sounds.

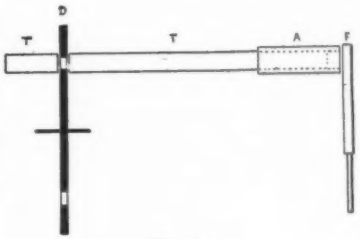

Fig. 8.

Experiments made with this apparatus gave the same results as those made with apparatus (A), but the sounds given are so feeble compared with those coming from the resonators (fig. 7) that the periods of sound and silence (or, rather of sound and much diminished sound) are not sharply separated. It followed that the judgment of a continuous sensation on the ear could not be so neatly made with the use of the resonant tubes as when the resonators were employed.

(C) To obtain sharper demarcation of sound and silence by having no aperture for the lateral escape of sound between the rotating disk and the nipple of the resonator and between the disk and the tube conveying the sound to the ear, I made the following apparatus, fig. 9.

I turned disks of brass flat and of uniform thickness. These disks were revolved on a rotator driven by gear wheels made of "fiberoid," so that the movement should be noiseless. The number of teeth on the wheels and holes in the disks were such that I was enabled to make three determinations corresponding in the number of interruptions of sound to those already obtained with apparatus (A).

Two brass tubes, T and T′, fig. 9, one having one interior diameter of $\frac{1}{2}^{cm}$, the other an interior diameter of 1^{cm}, slid accurately and with little friction in two tubes, A and B, with flanges on their ends. These flanges were pressed against the

surfaces of the disk D by two delicate helical springs fitting over the tube at S and S′, between the flanges of A and B and the standards P and P′. The tubes T and T′ were as close as possible to the disk while it rotated. The flanges A and B were of such diameter that no sound could issue between them and the surfaces of the rotating disk, because the flanges

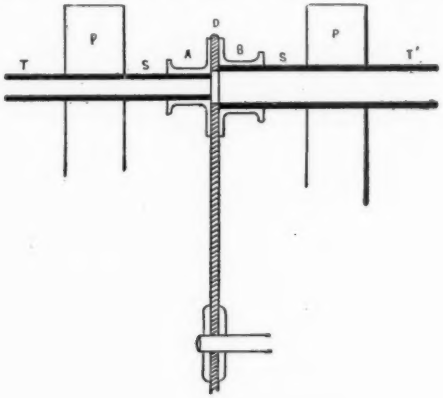

Fig. 9.

entirely covered the hole in the disk while this hole traversed the tubes T and T′. A tube of caoutchouc led from the tube T′ to the ear.

Although a film of oil was between the flanges and the disk and the disks were so accurately made that the greatest departure from uniformity of thickness amounted to only $\frac{1}{25}^{mm}$, yet the sounds produced by the sliding of the disk between the flanges caused much distraction in the perception of the sound from the resonator, which was adapted to the tube, T, of $\frac{1}{2}^{cm}$ in diameter. The results obtained with this apparatus agreed with those given by apparatus (A).

(D) I had formed great hopes of having the best apparatus for the determination of the duration of a residual sensation in the one shown in fig. 10. A is the lower drum of a Helmholtz double siren. D, the perforated disk of the siren, which was rotated by the driving-wheel W. The disk, D, was inclosed in the cover, C, of the form shown in fig. 10, clamped to the drum of the siren. The sound issued from the box, thus formed, and was conveyed to the ear by the tube E, to which was attached a tube of caoutchouc. The sound to be experimented on was conveyed from the fork, F, and resonator, R, through a long tube T to the drum of the siren. By placing pulleys, P, of various diameters on the axle of the

disk of the siren and by opening one or another of the various circles of holes in the drum, A, I had the means of obtaining a considerable range in the numbers of interruptions of sounds per second.

The results given by this apparatus were the same as those obtained with (A), but the objection to its action is the produc-

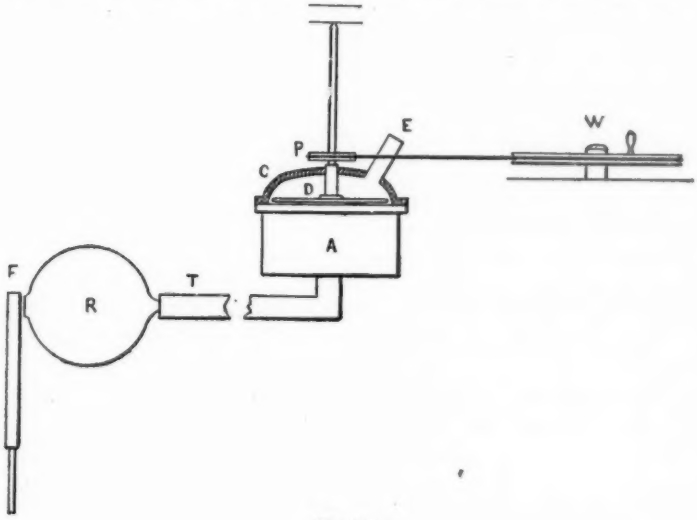

Fig. 10.

tion of sounds by the apparatus itself, caused by the rotation of the perforated disk. These sounds distracted the attention from the phenomenon of the continuous sensation produced by the interruptions of the sound from the fork and so masked it that I consider this apparatus the least efficient of any I have described.

These experiments on the blending of interrupted sounds are not pleasant to make. The ear soon becomes fatigued and the perception of sound is dulled. After an experiment the ear has to be rested during a considerable time before the experiment can be repeated satisfactorily. Thus much time is consumed and these experiments cannot be made in a few days, but weeks are required to arrive at satisfactory measures; also considerable time is consumed in gaining mastery over the apparatus. To make these experiments less tedious, fatigue of the ear is to be avoided. This is done by not allowing the interrupted sounds to enter the ear longer than during two or three seconds, then a rest of 5 to 6 seconds is taken while the

fork is kept in action and the disk revolved with the same velocity; then another three seconds period of sound is given the ear. This is best done by placing the rubber tube in the meatus of the ear and pinching the end of the tube between the fingers. By relieving the pressure more or less we can regulate the intensity of the sound which enters the ear, or we can shut the sound off. The other ear is tightly closed with beeswax softened with a little turpentine.

Within the limits of the intensities of sound used in these experiments I found no change in the duration of the sensation with change in the intensity of the sound. It seems probable that such connected changes exist. If they do exist, then it would appear, from the smoothness of the curve I have obtained from the experiments, that the relative mean intensities of the sounds used did not vary sufficiently to make apparent any change in the duration of the after-sensation with change in the intensity of the stimulus.

Table of results of experiments. The empirical formula which gives the relation of the pitch of a sound to the duration of its residual sonorous sensation.

The results of the experiments made with the various forms of apparatus just described are given in the following Table I.

TABLE I.

A	B	C	D	E	F	G	H	I	K	L
UT_1	64	23·1	$\frac{1}{2^6 \cdot 6}$	·0361	·0369	−·0008				1·38
UT_2	128	36	$\frac{1}{4^3 \cdot 2}$	·0231	·0228	−·0003	·0280	·0324	+·0044	1·77
SOL_2	192						·0232	·0237	+·0005	
UT_3	256	62	$\frac{1}{7^3 \cdot 4}$	·0134	·0133	−·0001	·0190	·0189	−·0001	2·06
Mi_3	320	73	$\frac{1}{8^3 \cdot 5}$	·0114	·0112	−·0002	·0160	·0158	−·0002	2·12
SOL_3	384	88	$\frac{1}{10^3 \cdot 6}$	·0094	·0097	+·0003	·0137	·0137	·0000	2·18
UT_4	512	108	$\frac{1}{13^3}$	·0077	·0078	+·0001	·0110	·0109	−·0001	2·37
Mi_4	640	126	$\frac{1}{15^3 \cdot 8}$	·0066	·0067	+·0001	·0092	·0092	·0000	2·53
SOL_4	768	143	$\frac{1}{17^3 \cdot 6}$	·0058	·0059	+·0001	·0080	·0081	+·0001	2·68
UT_5	1024	170	$\frac{1}{20^3}$	·0049	·0049	−·0000	·0066	·0066	·0000	3·01
Mi_5	1280						·0057	·0057	·0000	
SOL_5	1536						·0052	·0052	·0000	
Ut_6	2048						·0045	·0044	−·0001	
Mi_6	2560						·0039	·0039	·0000	
SOL_6	3072						·0036	·0036	·0000	

Column A gives the names of the sounds, and B the numbers of their vibrations (v. d.) per second. C, the number of beats of interrupted sound received by the ear in a second to blend into a continuous uniform sensation. In column D are the durations of the residual sonorous sensations expressed in vulgar fractions. The numbers in column D are obtained, as already explained, by diminishing the reciprocals of the numbers in col. C by $\frac{1}{6}$. Under E are the fractions in column D

in decimals. Under F are the durations of the residual sensations computed by the formula

$$D = \left(\frac{33000}{N+30} + 18\right) \cdot 0001$$

in which D = the duration of the residual sensation given by a sound of N number of vibrations per second. Under G are the differences between the computed and observed values of the residual sensations. These differences are so small, except in the case of SOL_2, that we may adopt the empirical formula as expressing the law, which connects the pitch of a sound with the duration of its residual sonorous sensation. This formula, however, only refers to my own auditory sensation. It is a physiological law and I imagine that the durations of these residual sonorous sensations will vary more with different observers than do analogous visual sensations. It is therefore to be wished that others will repeat these experiments and obtain determinations which, when combined, will give a law which may be accepted as the expression of the average durations of the residual sonorous sensations.

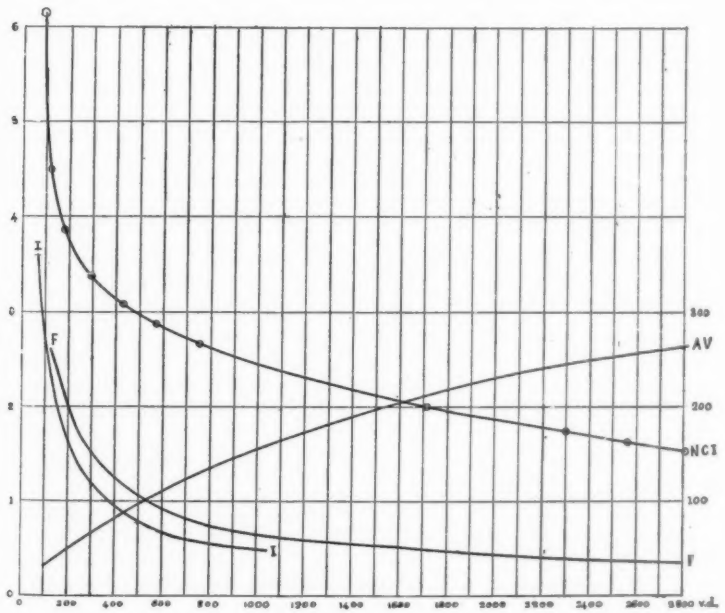

Fig. 11.

The results in column E are given graphically in the curve I of fig. 11. The unit on the axis of abscissas is 100 v. d. The unit on the axis of ordinates is ·01 second.

The determination of the duration of the residual sensation of UT_1 (64 v. d.) was very carefully made and made many times. The experiments gave ·0361 second for the duration of this sound. The formula gives ·0369, which is $\frac{1}{45}$ greater than the observed duration. The greatest difference existing between the observed and computed duration of the remaining sounds of the table is in the case of SOL_3 (384 v. d.) where the computed is $\frac{1}{31}\cdot\frac{3}{3}$ greater than the observed duration. In column L of Table I are given the number of wave-lengths of the sounds which pass into the ear through a hole in the rotating disks when these interrupted sounds blend into uniform sensations. The average number of wave-lengths which pass a hole is about $2\frac{1}{4}$. As the sound rises in pitch more wave-lengths pass the hole; thus while only 1·38 wave-length passes in the case of UT_1, 3 wave-lengths pass in the case of UT_6.

An examination of fig. 6 shows that sound passes to the ear while a hole in the disk passes over three diameters of the hole in the nipple of the resonator, while the distance between the centers of neighboring holes in the disk equals six diameters of the hole in the resonator; hence, to ascertain the number of wave-lengths which enter the ear during the passage of a hole in the disk across the hole in the resonator we must divide the number of vibrations per second of the sound, given in column B, by twice the corresponding number in column C.

2. *On the Smallest Consonant Intervals among Simple Tones.*

When two simple tones, which differ slightly in pitch, are sounded simultaneously, beats are produced, which become more frequent as the difference in pitch increases, and with this increase in the interval between the tones the dissonance becomes harsher, reaching a maximum of dissonance (when the number of beats are about $\frac{4}{10}$ of the number required to blend), then becoming less dissonant as the interval increases till, at last, the two tones blend into a consonance. These are the phenomena observed from SOL_1 (96 v. d.) to the highest tones used in music.

Having the law which gives the number of beats (produced by the interrupted sounds of tones of various pitch), which blend, one might naturally infer that the consonant interval could be computed by that law. Given the pitch of a tone we compute by the law the number of interruptional beats of this tone which blend, and adding this number to the frequency

of the given tone we should apparently have the pitch of the upper tone which makes with the lower the smallest consonant interval.* This however is not so. Take, for example, UT_3 (256 v. d.). The number of interruptional beats of this sound which blend is 62, and 256+62 = 318 which, according to the law, should make a consonant interval with UT_3. But experiment shows that a tone of 256+58 = 314 v. d. forms the smallest consonant interval with UT_3.

To render less tedious the comprehension of the results of many experiments on the smallest consonant intervals among simple tones I shall at once give a table (Table II) of the results of these experiments and then give the account of the experiments that furnished the data of the table.

In column A are given the lowest tones of the consonant intervals which were experimented on. In column B are the number of vibrations to be added to the tones of column A to form the higher note of the smallest consonant interval, as deduced from the experiments on the duration of the residual sensation of interrupted sounds. In column C the numbers of vibrations by which the tones in column A have really to be increased to form the higher notes of the smallest consonant intervals. In column D are the numbers of vibrations to be added to those of column A to form the smallest consonant intervals as computed by the formula

$$N : N + \frac{1}{\left(\frac{42500}{N+23} + 23\right) \cdot 0001}$$

in which N = the number of vibrations of the lower tone of of the interval and $N + \frac{1}{\left(\frac{42500}{N+23} + 23\right) \cdot 0001}$ = the number of vibrations of the higher tone of the interval. In column E are given the differences between the computed values (D) and the observed values (C). The formula gives quite closely the true values from SOL_2 (192 v. d.) to MI_6 (2560 v. d.). In column F are given the smallest consonant intervals, as determined experimentally, from SOL_1 (96 v. d.) to the tone of 2806 v. d., expressed in semitones of the equal-tempered scale.

In figure 11 these intervals, computed from the numbers in column C, are expressed graphically by the curve N. C. I. The units of ordinates (on the left of figure) are semitones and the units of abscissas are 100 vibrations. This curve

* Rigorously, we should take in the computation the number of beats which blend corresponding to a sound of a pitch which is the mean of the pitch of the lower and upper sounds of the interval.

TABLE II.

Number of Experiment.	A Tones.	B Number of additional v. d. to form the smallest consonant interval as deduced from the experiments on the residual sensations of interrupted sounds.	C Number of additional v. d. really required to form the smallest consonant interval.	D Number of additional v. d. required to form the smallest consonant interval according to the formula $N : N + \frac{1}{\left(\frac{42500}{N+23} + 23\right)\cdot 0001}$	E Differences between D and C.	F Smallest consonant intervals in semitones of equal-tempered scale.
(1)	SOL (48)	19·	no consonant interval	16·		none
(2)	UT_1^{-1} (64)	23·1	no consonant interval	19·5		none
(3)	SOL_1 (96)	30·7	41	26·3	−14·7	$6\frac{15}{100}$ semitones.
(4)	UT_2 (128)	36	38	32·8	− 5·2	$4\frac{1}{2}$ "
(5)	SOL_2 (192)	49·7	48 +	47·4	− 0·6	$3\frac{86}{100}$ "
(6)	UT_3 (256)	62	58	57	− 1·0	$3\frac{53}{100}$ "
(7)	Mi_1^- (316)	72	68	67·4	− 0·6	$3\frac{34}{100}$ "
(8)	LA_3^- (432)	91·2	85·4	85·9	+ 0·5	$3\frac{12}{100}$ "
(9)	RE_4^- (575)	113·3	107·4	106·3	− 1·1	$2\frac{95}{100}$ "
(10)	SOL_4^- (766·2)	137·8	129·8	130·1	+ 0·3	$2\frac{70}{100}$ "
(11)	LA_5 (1706·6)	219·3	210·4	212	+ 1·6	2 "
(12)	RE_6 (2304)	251·4	245	242·3	− 2·7	$1\frac{76}{100}$ "
(13)	MI_6 (2560)	262·8	256	253·5	− 2·5	$1\frac{64}{100}$ "
(14)	Fork (2806)	272	266	263	− 3·0	$1\frac{54}{100}$ "

shows in a striking manner the contraction of the smallest consonant interval as we ascend the musical scale; while SOL_1 (96 v. d.) requires a sound separated from it by $6\frac{15}{100}$ semitones to give the smallest consonant interval, SOL_6 of 3072 v. d. forms a consonant interval with a higher sound separated from it by only $1\frac{1}{2}$ semitones. The curve AV, of fig. 11, which has the same units of abscissas as curve N. C. I, and the units of whose ordinates, in numbers of vibrations, are on the right of the diagram, shows graphically the number of vibrations to be added to the tones, on the axis of abscissas, to obtain the smallest consonant intervals.

The experiments which form the basis of the statements given in Table II, I shall now describe.

Having but few forks below UT_3 in pitch, and those not numerous enough to determine with accuracy the consonant intervals, I requested my friend Dr. Rudolph Koenig, of Paris, to determine for me the smallest consonant intervals among sounds below UT_3 in pitch. The numbers in the table referring to tones SOL_{-1}, UT_1, SOL_1, UT_2, and SOL_2, experiments Nos. 1, 2, 3, 4, and 5 of Table II, were furnished by Dr. Koenig's experiments. The account of these experiments, which Dr. Koenig so obligingly made for me, I give in his own words.

Dr. Koenig's Experiments.—"Paris, le 21 Mars, 1893 . . . Je veux maintenant repondre a vos questions concernant la consonance des intervalles formés de notes graves et pas trop fortes, et faire d'abord cette remarque générale, que la perceptibilité des roulements et raucités produits par des battements, qui depend avant tout de l'intensité relative des deux notes primaires, parait être presque entièrement indépendant de leur intensité absolue, quand elles sont graves, tandis que le rôle de l'intensité absolue augmente avec leur hauteur. En effet, les resultats des observations faites avec des diapasons relativement faibles, et sans résonateurs, sont pour les octaves graves presque absolument identiques avec les résultats donnés par les gros diapasons et résonateurs du grand tonomètre, et publiés dans le tableau, page 113, des "Quelques Experiences d'Acoustique," tandis que pour les intervalles avec les notes fondamentales UT_2 et SOL_2, les différences des résultats devient déja sensible. Je pourrais aussi résumer ces faits en disant que l'influence de l'intensité absolue des deux sons primaires sur la perceptibilité des battements, qui est absolument nulle pour les battements lents, augment pour les roulements et raucités avec les nombres des battements qui les produit, car, pour example, les 8 battements de $UT_1 : RE_1$ sont aussi distinctment entendus avec des notes UT_1 et RE_1 trés faibles, qu'avec des

notes les plus fortes, tandis que le roulement des 32 battements de $UT_1 : SOL_1$, se fait beaucoup plus sentir quand les deux notes sont fortes, que quand elles n'ont q'une faible intensité.

Voici maintenant la liste de mes observations:

(1) *Intervalles avec la note fondamentale* $SOL_{-1} = 48$ v. d.

Les diapasons employés, du modèle de l'interrupteur universel, Cat. No. 253, ont des branches d'environ 9^{mm} d'épaisseur sur 15^{mm} de largeur.

On ne trouve dans toute l'octave pas un seul intervalle sans battements fortement perceptibles, soit séparément soit comme roulement. Le roulement parait le plus faible vers la sixte, $SOL_{-1} : MI_1$.

(2) *Intervalles avec la note fondamentale* $UT_1 = 64$ v. d.

Les diapasons employés du modèle, Cat. No. 234, ont des branches d'environ 10^{mm} sur 17^{mm}.

Tout se passe dans l'octave entière comme avec les gros diapasons avec resonateurs, seulement le roulement confus près de la quinte est faible et disparait presque devant les battements secondaires, mais il reparait au dessus de $SOL_1 + 4$ v. d. et devient plus fort en passant au roulement simple des battements supérieurs. A la quinte exacte, $UT_1 : SOL_1$, ou les battements secondaires manquent, on entend très bien le roulement des 32 battements, mais plus faible qu'avec des gros diapasons et resonateurs.

Les mêmes experiences répétées avec des diapasons à branches de 19^{mm} sur 15^{mm} donnent le même résultat.

En somme, ici aussi il n'y a pas d'intervalle consonant dans toute l'octave.

J'ai encore particulierèment examiné l'intervalle $UT_1 : MI_1$, mais on y entend le roulement des 16 battements même encore quand les sons premaires sont devenus déja si faibles qu'on ne les perçoit presque plus.

(3) *Intervalles ave la note fondamentale* $SOL_1 = 96$ v. d.

Diapasons employés à branches de 10^{mm} sur 17^{mm}.

$SOL_1 : LA_1$	10·6	battements	séparément	entendus.
" $: SI_1$	24	"	roulement	simple fort.
" : 124 v. d.	28	"	"	plus faible.
" $: UT_2$	32	"	"	très faible.
" : 134 v. d.				faible raucité.
$SOL_1 :$ 136 }				
" : 138 }	Peut être regardé comme consonant, mais			

à SOL_1 : 140 v. d. les battements secondaires deviennent déja sensibles et augmentent vit en intensité. Quand ils disparaissent à la quinte exacte.

$SOL_1 : RE_2$, celle ci fait entendre une raucité à peine perceptible. Au dessus de RE_2 les battements secondaires reparaissent pour disparaitre vers 148 v. d.

SOL_1 : 150 v. d. Il y a encore un peu de raucité mais on pouvait à la rigeur regarder les intervalles d'ici à.

SOL_1 : 166 v. d. comme consonants. Au desus de 166 v. d. le roulement des battements superieurs commence, et augmente en intensité, et à partir de 180 v. d. les battements supérieurs sont séparément perceptibles.

En resumé, il y entre la quarte et la quinte une petite étendue, et au dessons et au dessus de la sixte une plus grande, ou se trouvent des intervalles consonants.

(4) *Intervalles avec la note fondamentale* $UT_2 = 128$ v. d.

Diapasons employés pour UT_2 jusqu'à UT_3, avec branches de 10^{mm} sur 17^{mm}, pour la note fondamentale UT_2 avec branches de $7\frac{1}{2}^{mm}$ sur 14^{mm}.

Tout se passe à peu près comme avec les gros diapasons et résonateurs (Cat. No. 197), seulement le roulement de $UT_2 : MI_2$ n'est plus qu'un simple raucité, qui diminue vit pour disparaitre à 166 v. d.

$UT_2 : FA_2$ est consonant, et la consonance persiste jusqu' à 188 v. d. ou les battements secondaires commencent à la troubler jusqu'à la quinte, $UT_2 : SOL_2$, qui est consonant. Quand ils ont troublé la consonance des intervalles au dessus de la quinte jusque vers UT_2 : 197 v. d. les intervalles sont de nouveau consonants et le restent jusque vers UT_2 : 222 v. d. ou alors le roulement des battements superieurs commence.

En resumé, les intervables d'un peu au dessau de la quarte jusqu' à près de la quinte, la quinte, et les intervalles d'un peu au dessus de la quinte jusque au dessus de la sixte sont consonants.

Avec des diapasons du modèle de Cat. No. 38*a*, aux branches de 15^{mm} sur 20^{mm}, montés sur caisses, le roulement des 32 battements de $UT_2 : MI_2$ est fort ; $UT_2 : SOL_2$ est consonant, mais comme $MI_2 : SOL_2$ donne le meme nombre, 32, de battements que $UT_2 : MI_2$, en faisant sonner ensemble UT_2, MI_2, SOL_2, les 32 battements do $UT_2 : MI_2$ et de $MI_2 : SOL_2$ se renforcent et produisent un roulement formidable.

(5) *Intervalle avec la note fondamentale* $SOL_2 = 192$ v. d.

Diapasons employés avec branches de 10^{mm} sur 17^{mm}.

SOL_2 : LA_2 Roulement simple assez fort.
" : 244 v. d. Raucité prononcé.
" : SI_2 Raucite faible.

La consonance commence encore avant SOL_2 : UT_3.

Il resulte de l'ensemble de ces observations, que pour les intervalles des notes très graves les faits ne s'accordent pas du tout avec votre loi, mais aussi que pour les intervalles des notes de plus en plus aigues, l'accord entre les faits et la loi devient toujours meilleur et finit par être presque parfait quand la note fondamentale atteint SOL_2."

I experimented on the hearing of twelve persons with all the forks at my command among which I could obtain consonant intervals. It is true that the number of intervals thus furnished, by 80 forks, did not amount to many. Many such intervals can only be obtained in the laboratory of Dr. Koenig where is his "grand tonomètre" giving the frequency of all sounds from 16 to 21845 complete vibrations per second. However, I secured, between UT_3 and SOL_6 enough intervals among the forks to establish the law and the facts given in Table II.

All the persons experimented on, except myself and one other observer, were accomplished musicians, several of them violinists of more than exceptional ability. Three were graduates of the Conservatory of Music of Leipzig.

These experiments were all made in the same manner, viz: by taking a fork giving the lower tone and sounding it successively with others which gave more and more beats per second, till these beats blended into a continuous smooth sensation, forming the smallest consonant interval. As musicians rather avoid than dwell on dissonant intervals I educated each one in the special subject of the roughness of the sensation given by beats, by making the beats more and more frequent till near consonance, then giving an interval which is admitted by every one to be consonant. In this way their hearing was trained in what I wished them to discern, viz: that separation in the pitch of two forks which just gives a consonant interval.

The variation among the decisions of these different observers never equalled two vibrations, generally they agreed exactly. This agreement among observers in the judgment of a consonant interval is remarkable. I give the mean of these

experiments. The number of the paragraphs refer to the number of the experiment given in Table II.

(6) $UT_3 : MI_3 = 256 : 320$ gave a consonant interval. I narrowed the interval by lowering the pitch of MI_3 from 320 to 315, 314, 313·5 and 313

UT_3 : 315 consonant.
" : 314 just consonant.
" : 313·5 slightly rough.
" : 313 decidedly rough.

These experiments show that $256 : 256+58$ = nearest consonant interval.

(7) $MI_3 : SOL_3 = 320 : 384$ is decidedly rough. Separated the interval by lowering the MI_3 fork from 320 to 916 then
$MI_3 : SOL_3 = 316 : 384 = 316 : 316+68$ = smallest consonant interval.

(8) $LA_3 : UT_4 = 439 : 517·4$ decidedly harsh. Separated interval by lowering LA_3 of 439 v. d. to 432 v. d. then
$LA_3 : UT_4 = 432 : 517·4 = 432 : 432+85·4 =$ smallest consonant interval.

(9) $RE_4 : FA_4 = 576 : 682·65$ slightly rough. Increased the interval by lowering RE_4 of 576 v. d to 575 v. d. then
$RE_4 : FA_4 = 575 : 682·65 = 575 : 575+107·65 =$ smallest consonant interval.

(10) SOL_4 : Fork of 896 v. d. = 768 : 896 slightly rough. Increased the interval by lowering SOL_4 of 768 v. d. to 766·2 then
$SOL_4 : 896 = 766·2 : 896 = 766·2 : 766·2+129·8 =$ smallest consonant interval.

(11) $LA_5 : SI_5 = 1706·6 : 1920$ consonant. Narrowed the interval by lowering SI_5 of 1920 to 1917 then
$LA_5 : SI_5 = 1706·6 : 1917 = 1706·6 : 1706·6 + 210·4 =$ smallest consonant interval.

(12) $RE_6 : MI_6 = 2304 : 2560$ consonant. Narrowed the interval by lowering MI_6 of 2560 v. d to 2549 then
$RE_6 : MI_6 = 2304 : 2549 : = 2304 : 2304+245 =$ smallest consonant interval.

(13) Mi_6 : Fork No. $11_6 = 2560 : 2816 = 2560 : 2560+256$ is just perceptibly rough.

(14) Fork 11 : $SOL_6 = 2816 : 3072$ slightly dissonant. Increased interval by lowering Fork 11 of 2816 v. d. to 2806 then
Fork 11 : $SOL_6 = 2806 : 3072 = 2806 : 2806 + 266 =$ smallest consonant interval.

In the experiments just described the intervals of the tones that gave consonance were made by simple tones, of small intensity and free from the slightest trace of the upper partial tones of the forks, and the two forks were vibrated so that they gave, as near as I could judge, the same intensity of

sound. The results given only refer to intervals so formed. To obtain them the forks were gently vibrated by strokes of rubber hammers that varied in hardness with the pitch of the forks. The lower the pitch of the fork the softer should be the hammer. A hammer of hard rubber striking low pitched forks will develop the upper partial tones of the forks and so vibrate the experiments that a really consonant interval might be judged as dissonant.

The results of all the experiments may be summed up as follows: From SOL_2 of 192 v. d. to MI_6 of 2560 v. d. the smallest consonant intervals are closely given by the formula

$$N : N + \frac{1}{\left(\frac{42500}{N+23} + 23\right) \cdot 0001}$$

For sounds below SOL_2 the interval as computed by the formula is too small to agree with the true interval. For sounds above MI_6 (2560 v. d.) the intervals computed by the formula, like those below SOL_2, are too small. That the experimental determination of the smallest consonant interval throughout four octaves, upward from SOL_2, or, throughout the tones given by the violin, should agree so closely with the formula indicates the existence of a law connecting the magnitude of the smallest consonant interval with its position in the musical scale.

Dr. Koenig has shown that a consonant interval does not exist among simple sounds of pitch below SOL_1 (96 v. d.), yet I have found that the sound of UT_1 (64 v. d.), when interrupted by a rotating perforated disk, blends perfectly, to my ear, when these interruptions occur 23·1 times in a second. It may appear strange that although 23·1 interruptions per second of the sound UT_1 blend, yet a consonant interval does not exist throughout the interval of UT_1 till the interval $UT_1 : UT_2$ is reached; but the beats produced by the rotating perforated disks are produced by the interruptions of one tone, whereas, when two simple tones are conjoined two sets of beats are produced, inferior and superior; thus, when UT_1 forms an interval with UT_1+23 v. d., the inferior beats are 23 per second and the superior beats are 41 per second, and the interaction of these inferior and superior beats produces secondary beats which give to the interval a confused rumbling sound.* Of this interval, $UT_1 : UT+23·1$, Dr. Koenig wrote to me as follows: "Your 23·1 interruptions of UT_1 correspond, in number, to the inferior beats of the simple tones $UT_1 : UT_1 +$

*See Quelques Expériences d'Acoustique, par Rudolph Koenig. Paris, 1882, pp. 89, 107, 113.

23·1, but it is just at this magnitude of interval that the superior beats begin to assert themselves, to produce with what remains perceptible of the inferior beats, the confused rumbling, which evidently would be but a slight roughness (disappearing entirely at a further increase of the interval), if the superior beats, whose intensity from this point increases with the interval, did not exist."

3. *The Durations of the Residual Sonorous Sensations as deduced from the Smallest Consonant Intervals among Simple Tones.*

If we assume that two simple tones form the smallest consonant interval because the beats produced by these conjoined sounds have blended into a smooth continuous sensation, then we may deduce the durations of the residual sonorous sensations from the observed smallest consonant intervals in the following manner. The reciprocals of the numbers in column C, Table II, are taken as expressing the durations of the sonorous sensations given by tones whose numbers of vibrations are the mean of those of the lower and higher tones of the corresponding consonant intervals, for, when two sounds of different pitch blend, there is no reason why the duration of their residual sensation, as given by the reciprocal in column C, should refer to the lower sound more than to the higher. Therefore we have taken these reciprocals from column C as expressing the durations of sounds having the mean pitch of the two associated sounds forming the interval. The residual sensations thus found were projected in a curve, drawn to a large scale. From this curve were obtained the durations of the residual sensations of the tones of the musical scale. These durations are given in column H of Table I.

In column I of Table I are given these durations as computed by the formula

$$D = \left(\frac{48000}{N+30} + 21\right)\cdot 0001$$

In column K are the differences between the durations computed by the formula and the durations given in column H. These differences show that the formula expresses closely the durations of the residual sensations thus deduced from the determinations of the smallest consonant intervals, except in the case of UT_2; for which tone the computed number of vibrations to be added to it to form the higher tone of its smallest consonant interval, as shown in Table II, is 5·2 vibrations less than the number of vibrations really required.

In figure 11 these durations, as determined from the smallest consonant intervals are plotted in the curve F so that the com-

parison of the durations of the residual sonorous sensations thus determined may be readily compared with those given (by the curve I), of the residual sensations as determined by the blending of sounds interrupted by rotating perforated disks.

The ordinates of the curve I and F of fig. 11 are obtained in fractions of a second by changing the numbers 1, 2 and 3 on the left of fig. 11 into ·01, ·02 and ·03.

These two curves of fig. 11 present the same general character of a rapid upward flexure at the points corresponding to about 600 v. d.

The durations of the sound-sensations thus deduced from the smallest consonant intervals average about $\frac{1}{3}$ greater than those given by the beats of interrupted sounds. It may be supposed that the durations of the sonorous sensations deduced from the smallest consonant intervals of simple tones are greater than those determined by sounds interrupted by the perforated disks because in the resultant actions of the vibrations of the tones, forming the smallest consonant intervals, the periods of silence, or, of the periods of great diminution of sound, are a fraction of the periods of sound, or, of the periods of maximum intensity of sound. To test this opinion I combined the sinusoids corresponding to the two tones of various smallest consonant intervals. On taking, as the residual duration of the sound, not the time from maximum to maximum of vibration (as in the deduction of the durations from the smallest consonant intervals), but the interval of time during which much diminished intensity of sound exists, as shown in the combined curves, I found that the durations of sonorous sensations were thus reduced, on the average, about $\frac{1}{2}$, whereas, the reduction in time should be only $\frac{1}{3}$ to make these durations agree with those determined by the rotating perforated disks. The explanation suggested is therefore not tenable.

For the period of much diminished intensity of sound I took that length (in time) of the resultant curve which is bounded, at each end, by an amplitude of vibration $\frac{1}{2}$ of the maxima amplitudes of the curve. We are here in doubt as to the relative intensities of the sensations given by two sound-vibrations whose amplitudes are 2 : 1 and whose energies are 4 : 1. We at once face an obstacle which, from our want of knowledge, is insurmountable; for, assuming that either the law of Weber, or the formula of Fechner deduced from it, correctly gives the relations existing between the intensity of a stimulus and its corresponding sensation, we cannot apply either of these laws, because we do not know the *absolute energies* of the sound-vibrations whose sensations are to be

compared. Thus, if we adopt the law of Weber, with the least perceptible difference in the sensation of two sounds equal to $\frac{1}{3}$ of their energy, as given by the experiments of Volkmann,* we find that if 1 and 4 are the absolute energies of the sound-vibrations we get for the ratio of their corresponding intensities of sensations 1 : 2·6, but, if the absolute energies of the sounds are 10 to 40 (and their ratio is also 1 : 4), we get for their relative sensations 1 : 1·48. Or, what is the same, on the curve expressing the law of Weber, or, of Fechner, the ratio of the sensations of two sounds as given by their corresponding ordinates, depends on the number of units on the abscissas forming the ratio of the energies of these sounds.

Professors Cattell and Fullerton from extended experiments "On the Perception of Small Differences,"† very carefully

* In the investigations, of which I have knowledge, the experimenters have used either noises, or, sounds of complex composition mingled with noise, and the ways in which they have determined the relative energies of sounds, or, of noise-producing vibrations, are open to criticism. I do not know of similar experiments made with simple sounds, or, tones. I would suggest that the problem of determining the differences in the energies of two simple sounds to give a perceptible difference in the sensations they cause may be solved as follows: A fork, or, rod, is vibrated with a constant amplitude and this amplitude is measured with a micrometer-microscope. A second fork, or, rod, placed alongside of the first fork, or, rod, has a much smaller amplitude of vibration, which can be varied, and is also measured with a microscope. The second fork differs from the first slightly in pitch, so that, say, three beats per second are given. The amplitude of the second, or, of the first fork, is varied till the perception of beats just vanishes or just appears, while the ear is kept at a fixed distance from the forks. If we take for the relative energies of the sound-giving vibrations the ratio of the squares of the amplitudes of the forks, the least perceptible difference in sensation corresponding to the differences in the energies of the sounds may be computed. As example: suppose the second fork has $\frac{1}{10}$ of the amplitude of vibration of the first. Then the energy of the maximum sounds of the beats will be $\overline{20+1}^2 = 441$, and the energy of the minimum sound of the beating will be $\overline{20-1}^2 = 361$, and $\frac{441}{361}$ = the ratio of the stimuli giving the least perceptible difference in sensation. Sound vibrations of different amplitudes and of different pitch will have to be experimented with and the fork giving the greater amplitude of vibration should, in successive experiments, be lower in pitch and then higher in pitch than the fork giving the lesser amplitude of vibration, for reasons set forth in my research. "(1) On the obliteration of the sensation of one sound by the simultaneous action on the ear of another more intense and lower sound. (2) On the discovery of the fact that a sound even when intense cannot obliterate the sensation of another sound lower than it in pitch." (This Journal, Nov., 1876: Nature, Aug. 10, 1876). Such a research will be difficult and tedious and will require many precautions in arranging the experiments.

Any one may readily observe the phenomena described by sounding a fork with a large amplitude of vibration and gradually bringing up to the ear a second fork with a small amplitude of vibration, giving with the first three beats per second. As the latter fork gradually approaches the ear the beats become stronger, reaching a maximum of intensity, and then diminishing till they vanish in the more intense sensation of the more intense sound, to reappear when the faintly vibrating fork has been brought closer to the ear.

† Publications of the University of Pennsylvania. Philosophical Series, No. 2, May, 1892.

made and skillfully reduced, have formed the opinion that neither the law of Weber, nor Fechner's formula is correct, and *a priori* considerations lead them to the opinion that it is probable that the sensation is directly as the stimulus. If the sensation increases directly as the stimulus, then we can obtain the relative sensations of two sounds whose relative energies are known. Adopting this relation, we have 1 : 4 as the ratio of the maximum sensation in the periods taken for those of much diminished sound to the maximum sensation in the periods of much increased sound, as given by the measurements of the amplitudes of the resultant curves of the nearest consonant intervals.

In explanation of the facts and laws given in this paper I have no hypothesis to offer. It appears to me that the present condition of our knowledge of audition demands that we should ascertain more facts relating to it before we frame hypotheses on the mechanism and action of the apparatus of hearing.

Stevens Institute of Technology, Hoboken, N. J.

Art. II.—*Petroleum in its relations to Asphaltic Pavement;* by S. F. Peckham.

[Read at the World's Congress of Chemists, Chicago, Ill., Aug. 25th, 1893.]

The chemical examination of petroleum has been conducted along two different lines of research. Sometimes one has been followed to the exclusion of the other and sometimes the two have been combined. The method most familiar to chemists is that of distillation, usually more or less destructive. The less well known method consists in separating the constituents of the petroleum by the successive application of solvents, in the use of which term I propose in this paper to exercise considerable latitude.

The earliest analysis of petroleum is described by de Saussure in a paper published in the Bibliothéque Universelle in 1817.* He examined the so-called naphtha of Amiano, a very light and almost colorless petroleum. He purified it by distillation and determined the percentage composition, as if it were an homogeneous substance. Twenty years later, in 1837,† Boussingault published his celebrated memoir on the composition

* Bibliothéque Universelle, iv, 116; Annales de Chemie et de Physique (2), iv, 314–340; London Journal of Science, iii, 411.

† Annales de Chemie et de Physique (2), lxiv, 141; Journal of the Franklin Institute, xxiv, 138; New Edinburg Philosophical Journal, xxii, 97.

of bitumens, in which he described his manner of separating, partly by distillation and partly by solution, what he supposed were two homogeneous substances from the petroleum, or more properly maltha, of Bechelbronn on the lower Rhine. He found that in making the separation by distillation it was necessary to keep the material heated in an air bath for from 45 to 50 hours, and in a note makes this pregnant observation: "By this method it is impossible to estimate the two principles of the bitumen; as at this temperature (250° C.) a part of the petrolene is oxidized and passes into the solid state, or asphaltene." These two substances, the petrolene or liquid part, and the asphaltene or solid part into which nearly all bitumens may be resolved in varying proportions, have not received the attention which they deserve, partly because as chemical compounds they are without significance and partly as they are separated from different bitumens, the petrolene in particular, they have very unlike properties.

On visiting southern California in 1865,* all the natural phenomena attending the passage of petroleum through maltha into asphaltum, were witnessed upon a very extended scale, and I was forced to consider as matters of economic importance, many problems that had not hitherto been brought to the attention of technologists. In what manner the changes, everywhere proceeding upon the surface were related to possible changes below the surface, led me to imitate in every respect, save the element of time, the operations of nature as far as possible. I therefore proceeded upon a synthetic rather than an analytic line of investigation, and treated the unchanged oil issuing from deep-seated strata, to the action of air and ozone. As the action of these reagents might be explained as due either to the addition of oxygen or to the substraction of hydrogen, I tried the effect of chlorine and found it to be in the main identical with that of the first named gases. The product of these reactions was a brilliant solid, apparently identical with pure asphaltum, and also with Boussingault's asphaltene.

In 1870, I stated the problem to Prof. J. D. Whitney as follows: "To discover if possible whether asphaltum is formed from petroleum by a change that adds oxygen or substracts hydrogen or both? To subject several typical varieties of petroleum to the action of ozone under conditions as nearly identical as possible. To study the residues of such action and establish the identity (if such identity exists) between the solid residues and native asphalts. To study the liquid and volatile products of such decomposition."

* Geological Survey of California, Geology, ii, Appendix, p. 73; Reports 10th Census U. S., vol. x, Petroleum, p. 185.

Since 1870 Le Bel and Muntz,* (1872) have treated Syrian asphalt and several other forms of bitumen to the solvent action of ethylic-ether. Ethylic-alcohol was then added to the solution and the bitumen precipitated. The solution and precipitation were repeated until the precipitated bitumen was quite pure. It appears as a brown powder. From solution in carbon disulphide it appears in brilliant black scales, which are insoluble in light petroleum naphtha (paraffines) but dissolve in the heavy distillates, which fact readily accounts for the difference in color between the light and heavy natural oils. These authors remark that, "Its (asphaltene) presence in natural bitumens proves that they have not been submitted to distillation, nor even the action of a high temperature, which corroborates the most generally received opinion respecting their origin, that they have been directly derived from coal or lignite."

Subsequent research by Le Bel showed that a resinous substance remained dissolved in the ether that was readily acted on by sulphuric acid. Distillation converted it into a residue of carbon, light oils and gas. These light oils when treated with iodohydric acid yielded compounds of the olefine series.

Later, Cabot† discovered that paraffines subjected to the action of sulphur at temperatures above their boiling points were converted into hydrogen sulphide and carbon. At about the same time Jenney,‡ aspirated air continuously for forty or more hours, through refined illuminating and lubricating oils, in presence of litharge and at a temperature of 100° C.–140° C. By this means he converted a portion of the oil into a solid oxidized residue, a portion distilled into the receiver, and a portion remained in the retort. These residues were either brilliant black solids or brownish flocculent precipitates, which were all soluble in carbon disulphide.

Lastly, I have lately found that a heavy fraction of a distillate, that was a transparent mobile fluid, obtained by distilling California petroleum at nearly a red heat, when left to stand in an open Becker glass for from 12–14 months, had become so charged with asphaltene, that the addition of petroleum naphtha or any of the liquids in which asphaltene is insoluble, at once produced a brown flocculent precipitate. This observation completely contradicts the conclusion reached by Le Bel and Muntz, and stated above, that natural oils containing asphaltene could not have been distilled or subjected to high temperatures.

* Bulletin de la Société Chemique de Paris, xvii, 156.

† American Chemist, vii, 20; Chemical News, xxxvi, 114; Wagner's Berichte, 1876, p. 1110.

‡ American Chemist, v, 309; Wagner's Berichte, 1875, p. 1060.

These researches show that the soluble and fluid portions of bitumens are converted into insoluble and solid forms, that may or may not contain oxygen, either by prolonged action of air and other oxidizing agents at ordinary temperatures, or at higher temperatures. They also show that the same effects are produced without the action of reagents by prolonged heating alone and the familiar process in technology known as "cracking," is a further illustration of the tendency of bitumens under the influence of heat alone, to break up into one series of compounds richer than the original in hydrogen and another compound or series of compounds richer than the original in carbon.

Now within a few years and since all of this work was completed, the laying of asphalt pavements has become an industry involving the expenditure of vast sums of money and consequently presenting technical problems of vast importance. The technology proceeds about as follows: A quantity of asphaltum is brought to New York from Trinidad. This asphaltum consists of water, 27 per cent; inorganic matter, consisting of fine aluminous sand, 27 per cent; organic matter insoluble in carbon di-sulphide, 8 per cent; bitumen 38 per cent. This crude asphaltum is put into large open kettles and melted, the temperature being raised and maintained for some time in the neighborhood of 400° F. By this treatment the water and lighter oils are expelled, and a portion of the mineral matter sinking out of the melted mass to the bottom of the kettles, a "refined pitch" is left, which is drawn off. This "refined pitch," consists in round numbers, of 56 per cent of bitumen, 36 per cent of mineral matter and 8 per cent of organic matter not bitumen. Of the 56 per cent of bitumen, 36 per cent is soluble in petroleum naphtha, leaving 20 per cent that may be obtained from solution in carbon di-sulphide in brilliant black scales. Properly interpreted the refined pitch consists of:

Petrolene			36 per cent.
Asphaltene	20 per cent.		
Organic matter not bitumen	8 "		
Mineral matter	36 "		
	64 "	36 "	

64 per cent of material without cohesion; that is, dry solids for the most part, are held together by 36 per cent of a viscous adhesive tar. The 20 per cent of the asphaltene dissolves in the petrolene, the remaining 44 per cent being simply mineral and organic matter held together by the combined bitumens.

The next step in the process is the addition to the refined pitch of about 15 per cent of its weight of some heavy petroleum residuum, which renders the bitumen softer, and at the same time may or may not dissolve both petrolene and asphaltene. The mixture is made complete by blowing air through the melted mass for hours or even days. Sand is then added to the bituminous cement until the bitumen is only equal to 10 per cent of the mass. To incorporate the sand with the viscous mass, both are heated very hot and the sand and bitumen thoroughly mixed, when the mastic thus produced is put in its place and rolled.

Very great diversity is observed in the durability of the mastic thus prepared and laid, and to account for these diversities Mr. Clifford Richardson* would have us believe that it depends on the excavation on the Island of Trinidad from which the crude asphalt was taken. So too the Hon. Commissioner of Public Works of the City of New York, by the advice of Stevenson Towle,† Consulting Engineer, has reached a similar conclusion. Speaking of the Eighth Ave. pavement, a part of which "showed indications of disintegration" before it was accepted Mr. Towle says, "the asphalt used in this work was submitted by the contractor and analyzed and approved by chemical experts;" and yet, farther on in this same report it is stated that the asphalt was condemned because it was taken from one excavation on the island rather than another.

It is a very significant fact that an asphalt pavement should show signs of disintegration within a few weeks of the time of its having been laid. Especially is this the case when it is considered that some of the most enduring constructions of antiquity, which have withstood the ravages of time for at least 3000 years, are constructions of asphalt. Modern constructions of asphalt in Europe are said to remain intact for more than twenty years. The asphalt pavement laid upon Franklin Avenue in the City of Buffalo, has stood, with almost no expense for repairs, for fifteen years and I do not believe any one now knows from what exact spot the asphalt came. It is a very well known fact however that the material used to soften the asphalt was one that would dissolve both constituents of the bitumen, and form a chemical union or solution rather than a mechanical mixture with them. No one knows how old the asphaltic sandstones are that contain 10 per cent of bitumen, but they do not disintegrate; they are solid, impervious and tough.

* Report of the operations of the Engineer Department of the District of Columbia for the fiscal year ending June 30, 1892, pp. 96–123.

† Report of the Department of Public Works of the City of New York on Street Pavements, with special reference to Asphalt Pavements. Thomas F. Gilroy, Commissioner. 1892. pp. 9, and 11 et seq.

If sand could be cemented together with carbon, it would only be necessary to grind up anthracite slack and mix it with sand in order to produce a paving material. It is evident that the hydrogen combined with the carbon gives it adhesive properties, and this within certain limits, for the moment a sufficient amount of hydrogen is removed to convert the petrolene into asphaltene, or whenever the softening material is a petroleum or residuum that will not dissolve asphaltene, the elements of disintegration are present.

The observation made by Boussingault more than half a century ago, and quoted above, that prolonged heating at high temperatures, converts petrolene into asphaltene, which has no more adhesive properties than anthracite slack, appears to have been overlooked by Messrs. Richardson and Towle, for they do not appear to regard the manner of refining and blowing the asphaltic cement as of any importance. Neither for the same reason do they appear to appreciate the bearing of my own researches or those of Messrs. Cabot and Jenney upon this question, although they have been on record now more than twenty years. While it has been well known for years that bitumens occur in great variety, the selection of a proper material for softening the asphalt to the exclusion of others less desirable or wholly unfit, appears also to have escaped attention. A properly selected material for such a purpose would by entering into solution and chemical union with both the constituent parts of the bitumen of the asphalt, thereby increase its adhesive and binding properties upon the other constituents of the mastic. Added to all these omitted elements of the problem just enumerated, we have another of great importance, and that is the total proportion of bitumen to the total proportion of sand and other non-bituminous ingredients of the mastic. Experience proves that less than 10 per cent–11 per cent to 90 per cent–89 per cent gives too little stability to the mass, while a larger percentage of bitumen makes the pavement too soft.

One more element of a good asphaltic pavement is mechanical rather than chemical, and that is solidity. The pavement must be solled as solid as asphaltic rock in order to keep out rain water and the action of the oxygen dissolved in it; for the effect of oxidation is to gradually convert the petrolene of the asphalt into ashpaltene, leaving the small amount of softening material as the only binding constituent of the mixture. When this softening material is not a solvent for asphaltene the pavement inevitably disintegrates.

I would therefore commend to the consideration of those who act as advisers to Commissioners of Public Works, not a

less careful examination of the asphalt used, from whatsoever source it may be obtained; but in addition an equally careful exercise of judgment regarding the softening material used, the chemical effects of refining and blowing, with careless management of either, and the composition and mechanical structure of the surface pavement.

It is far from complimentary to this age of scientific achievement that the ancient world should have furnished constructions of asphalt that have survived to the present, centuries after their builders are forgotten, while with all our superfluities of refining and blowing, with penetrating machines and chemical analysis, of which they knew nothing, we are only able to prepare constructions that endure but a few months or even years, at longest.

I am aware that this paper is inconclusive, but from the nature and present status of the problem it must be so. I think, however, it is, as it was intended to be, suggestive.

University of Michigan, Ann Arbor, Mich., Aug. 16, 1893.

ART. III.—*The age of the extra-moraine fringe in Eastern Pennsylvania;* by EDWARD H. WILLIAMS, JR.

AT the Madison meeting of the Geological Society, the writer called attention to the existence of this deposit, and claimed that it was a true "fringe" and of recent origin. Subsequent study has afforded the material for this paper, which will attempt to prove that, as far as Eastern Pennsylvania is concerned, the extra-moraine fringe is of extremely recent origin and, as it antedates the formation of the great moraine, all glacial deposits in this region are of extreme recency.

A section of the formation near Bethlehem will be taken as typical of the average for the region at the same level. It shows a surface clay, unstratified and carrying rolled stones from the Blue Ridge and beyond (Oneida, Medina, Oriskany, Helderberg, Marcellus, Hamilton, etc.), and covering a till that varies widely in its nature; but is mainly of local stuff of angular shape mixed with a greater or less proportion of rolled material similar to that in the clay and, locally, with angular masses of the same. This latter rests uniformly on glaciated rock in place. This section may vary by the omission of the till, or by the introduction of local stratified deposits of sand and clay. In every instance noted, and in thousands of instances, the pre-glacial surface soil has been removed, and the subsequent deposit rests unconformably on

the eroded country rock. As the ice-extension that produced the erosion antedated the formation of the great moraine, so also the upper clay is of the same age or younger than that formation. We have, therefore, a section that gives the extreme limits of the ice age in this region. In studying the till we notice—

First. The admixture of fresh and oxidized material at all levels, and the uniformity of oxidation at any point along a vertical section. The oxidized portion consists of angular masses of all sizes; or of angular fragments composing a ground moraine, and of fragments weathered to a spheroidal shape. The fresh portions are sub-angular and glaciated masses; or rolled bowlders, cobbles, pebbles and gravel. The latter are not found at a uniform level in the deposits; nor do they occur in a uniform proportion, as sections miles in length have contained but half a dozen of these foreigners and immediately adjacent to, and continuous with, this formation the foreigners would form an appreciable proportion of the whole deposit. This curious and persistent admixture proves that oxidation preceded glaciation and, while it may be taken as an evidence of the presence of part of the formation at or near the surface, it can in no sense be used as a criterion of the lapse of time since it was moved to its present position and mixed with the fresh and unoxidized part. This lapse, on the contrary, is to be estimated by the state of the fresh portion of the admixture.

Second. The following rule is universal. On going in the track of the glacier southward across the outcrops of slate, limestone and sandstone that make the floor of the great valley, we meet the ground moraine composed mainly of comparatively fresh fragments that, as in the case of the slate, pass gradually into the crushed and bent parts of the outcrop and rest on the solid rock. As we go to the south the slaty fragments become more decomposed and, as we pass from slate to limestone, they are rotted to a clay that retains, however, the cleavages of the small fragments. In the same way the till on the northern part of the limestone belt gradually loses its limestone fragments and retains only the chert at the south. The moraines south of the South Mountain have their southern border formed of weathered gneiss, while the Blue Ridge fresh rocks increase in proportion toward the northern border. This shows that the pre-glacial surface was scraped off by the glacier and exists along the southern part of the formation, where it is mixed with fresher local and foreign material.

Third. The surface outcrops, and those capped with clay, of gneiss, sandstone, slate and limestone are almost universally fresh and undecomposed. In the majority of cases there has

been no change of color from oxidation. No matter what the dip and strike may be, they come sharply to the surface and the cap is usually unconformable, except where a steep slope has produced a local creep. For examples see Preliminary Reports, 2d Geol. Surv. Penna., DD, Plate I; D3, vol. i, Plates II and III. These show that the till and clay did not oxidize after deposition; but that the already oxidized deposit was laid upon a freshly glaciated surface. It also shows that the date of glaciation is so recent that the gneiss, limestone and slate have had no time to decompose or oxidize. In other words, the glaciation has been quite recent. There are thousands of such outcrops, and they exist at all elevations between 220–650 A. T., and under caps that vary from loose gravels to compact clays.

Fourth. There seems to have been but a short interglacial period—if any existed at all—as the surface clay in many cases shades gradually into the till; or lies in a highly oxidized state upon a glaciated surface of absolutely fresh country rock.

Fifth. There has been an unbroken continuity in the till from the crest of the Blue Ridge, west of the Lehigh Gap, to the South Mountain. It has been followed down to and under the flood plain deposits of the present river systems of the region. It has been a favorite amusement with some to argue about the length of the interglacial period from the standpoint of the time necessary for the Lehigh to erode a channel from a so-called bed over Bethlehem hill, at 340 A. T. to its present level at 220 A. T. It might have saved a great deal of bother if it had been ascertained that the Lehigh was flowing on a rock bottom. It has been ascertained that a deep gorge runs along the north side of the South Mountain (whether continuous or not is not yet determined) of extremely narrow section and with a bottom of fresh limestone at depths varying from sixty to over one hundred feet below the present surface, and this has been traced from Easton to within seven miles of Reading, or from the Delaware to the Schuylkill. The limestone on the north side drops almost vertically down to the maximum depth, and the south side rises at a sharp angle. The section of the deposits filling this depression *is identical in succession with those of the rest of the valley.* In other words we have a depression that is glacial or pre-glacial, and is filled with the same succession of sands, clays, till, etc., that cover the rest of the valley; but the individual members of the deposit (excepting the top one) are from five to seven times as thick. The post-glacial erosion of the Lehigh and tributaries is a myth.

There is but one conclusion to be derived from the above. Granting that all the glacial formations (infra- and extra-moraine) represent the whole work of the ice age in North

America, and that we have them all in Eastern Pennsylvania, we can conclude that the total time from beginning to end was small, and of so recent a date that the streams—though resting on beds of clays—have not not reached in all cases their pre-glacial bottoms, and the exposed rocks—in spite of greater or less exposure to the atmospheric agencies—have not had time to acquire signs of decomposition or even oxidation. In fine, we seem to have had but one ice age, and that a short and recent one.

ART. IV.—*Notes on the Cambrian Rocks of Pennsylvania, from the Susquehanna to the Delaware;* by CHARLES D. WALCOTT.

IN a former paper a report was made of an examination of the lower Paleozoic rocks,* from the Susquehanna to the Potomac, and now attention is called to some observations, made during the past field season, relating to the basal quartzites and limestones of the lower Paleozoic rocks that extend across Pennsylvania, from the Susquehanna river to the Delaware river and across New Jersey to Orange county, New York, on the north, and into Chester county, Pennsylvania, on the east.

In the paper mentioned it is stated that *Hyolithes communis* and fragments of Olenellus were recognized in the material collected from limestones in Lancaster county, on the east side of the Susquehanna river,† and that, from the closely related stratigraphic arrangement of the rocks of Lancaster county, it is probable that all the Lancaster limestones will fall within the Cambrian, unless it be that some portion of the upper series may pass into the Ordovician. This generalization will also apply to the limestone in the adjoining counties of Berks and Chester, and, in fact, to the entire extension of this series northeastward, to the Delaware. All of the quartzites that have been referred to the Potsdam will necessarily fall into the lower Cambrian, as they are beneath the limestones.‡

Prof. A. Wanner, of York, Pa., accompanied me in the examination of the limestones about the city of Lancaster. His familiarity with the positions of the quarries and natural outcrops enabled us to make a rapid examination of the limestones along Conestoga creek, and, although no fossils were found, I saw no reason to think that the limestones were not of Cam-

* Notes on the Cambrian rocks of Pennsylvania and Maryland, from the Susquehanna to the Potomac, this Journal, vol. xliv, 1892, p. 469.

† Loc. cit., p. 474.

‡ Loc. cit., p. 475.

brian age. We next proceeded to the eastern side of the county, where the southern division of the limestone passes into Chester county. An examination was made of the lower quartzites and the superjacent limestones at Gap, Limeville and towards Compassville. A few Scolithus borings were noted in the quartzites northeast of Gap, and fragments of Olenellus and specimens of Obolella were found in sandy layers embedded in a shale one mile N.NE. of Gap. The section from the top downward, from a point a little east of Gap northwest, is:—

5. Massive-bedded, light-colored limestone with partings and small, interbedded, flattened nodules of mica schist.
4. Narrow belt of hydromica schist, with thin layers of hard calcareous sandstone containing Obolella and fragments of Olenellus.
3. Massive beds of bluish-black and nearly white limestone, extensively quarried at Limeville.
2. Narrow belt of shale altered to a hydro-mica schist.
1. Quartzite, in the hill northeast of Gap.

We next visited the extensive quarries at Bellemont, on the main line of the Pennsylvania railroad, four miles west of Gap. In the quarries the conglomerate limestone, so characteristic of the Cambrian at Stoner's station in York county, are beautifully exposed, and numerous photographs were taken of the conglomerate beds, showing their positions between the evenly-bedded limestones. The limestones and conglomerates belong to (3) of the Gap section, and are overlain by a belt of shale in which massive beds of a fine, limestone conglomerate occur. No time was given to searching for fossils.

The discovery of the Olenellus fauna in the limestone in the eastern portion of Lancaster county, north of Gap, taken in connection with the eastern section in York county, compels the reference of the so-called Potsdam rocks of Chester county with their superjacent limestones to the Cambrian. As mentioned in the previous paper, it is quite probable that the limestones towards the Triassic area, in the northern portion of Lancaster county, may be of Ordovician age, but this can only be proven by the discovery of the fauna.

Northern belt of Limestone.—As shown by the geological map of Pennsylvania (1884), the northern belt of limestone enters the State in Franklin county and then turns to the northeast in Cumberland county, crosses the Susquehanna at Harrisburg and extends on eastward across Dauphin, Lebanon, Berks, Lehigh and Northampton counties, to the Delaware river. The Olenellus fauna was found in the quartzites of South mountain in Adams county, as well as in the lower por-

tions of the limestone in Franklin county.* From this and the fact that the lower portions of these limestones and the superjacent quartzites or sandstones were known to be of lower Cambrian age in their extension into New Jersey, I began the examination of them in the vicinity of Reading, where their relations to the Reading sandstone are well defined. The *Scolithus linearis* occurs abundantly in the quartzites of Penn mountain, east of Reading, and on the south side of Neversink mountain across the Schuylkill. In the upper layers of the sandstone or quartzite I obtained *Hyolithellus micans* and fragments of a species of Olenellus. This locality is about one-half mile above Klappenthal station, on the Philadelphia & Reading railroad, and the terminal station of the electric road running out of Reading over the Neversink mountain. West of the first railroad cut above the station thinner beds of quartzite are met with, between the cut and the stone-crusher that carry fossils. The quartzites are more or less contorted and folded, but their stratigraphic relations to the main quartzites of the mountain and to the adjoining limestones are readily determined. No fossils were observed in the limestones.

The sections in the vicinity of Allentown and Bethlehem were examined and found to be essentially the same as at Reading, the quartzites passing beneath the limestone. In the quarries at Catasauqua, four miles north of Allentown, I noted the occurrence of a large Pleurotomaria, of the type of *Pleurotomaria canadensis*, of the Calciferous horizon in New York and Canada. In the report of the Second Pennsylvania Geological Survey of Lehigh and Northampton counties the localities of fossils are all on the northern side of the outcrop of limestone, in the strata that dip to the northward beneath the superjacent shales. The Trenton horizon is represented in the limestone immediately beneath the shales (called "Hudson"), and lower down, in massive limestone, species of Maclurea or Euomphalus have been found that indicate the Chazy horizon. This distribution of faunas is the same as in Franklin county, where the lower Cambrian fauna occurs at the base of the limestone near the quartzites, and the Trenton fauna at the summit, near the base of the superjacent shales.

The limestones were examined in the vicinity of Easton, along Bushkill creek and the shores of the Delaware river. No fossils were found with the exception of a species of Cryptozoan: but from the similarity of the limestone to that of the lower portion of the series near Allentown, Reading and in Lancaster country, it is quite probable that they represent the lower or Cambrian portion of the section. Five or six miles north of Easton, in the vicinity of Churchville, fossils were

* Loc. cit., p. 478.

discovered by the Pennsylvania survey that indicate the Trenton horizon. I think it is only a matter of detailed search and patience to discover localities of fossils, both in quartzites and limestones throughout this belt, from where it enters the state from Maryland and crosses the Delaware into New Jersey.

If we follow on the line of the strike of the limestones across the Delaware into New Jersey, the same type of section is found to extend northeasterly across the state and into Orange county, N. Y. At Hardistonville, Sussex county, New Jersey, Dr. Beecher discovered the Olenellus fauna in the blue limestones resting on the basal quartzite.* The fossils are found on the southeastern side of the limestone belt; and on the northwestern side the limestones dip beneath the shales as in the Pennsylvania section. In the geological report of New Jersey for 1868, pp. 131, 132, numerous localities of the fossiliferous Trenton limestone are described, and a section given showing the limestone passing beneath the shales to the westward.

The discovery of the Olenellus or lower Cambrian fauna in the Reading sandstone practically completes the correlation of the South mountain, Chickis and Reading quartzites of Pennsylvania and establishes the correctness of the early correlations of McClure, Eaton, Emmons, and Rogers. They all considered the basal quartzite as the same formation from Vermont to Tennessee; and the discoveries of recent years have proven that the basal sandstone of Alabama, Tennessee, and Virginia (Chilhowee quartzite); Maryland, Pennsylvania and New Jersey (the Reading quartzite); New York and Vermont (Bennington quartzite); were all deposited in lower Cambrian time, and that they contain the characteristic Olenellus fauna throughout their geographic distribution. The superjacent limestones carry the Olenellus fauna in their lower portions, in northern and southern Vermont, eastern New York, New Jersey and Pennsylvania. To the south of Pennsylvania the lower portions of the limestones appear to be represented by shales, and the upper and middle Cambrian faunas are found in the lower half of the Knox dolomite series of Tennessee, and they will probably be discovered in the same series in Virginia and Maryland, when a thorough search is made for them. The same may be predicted, but with less assurance, for the Northern belt of limestone crossing Pennsylvania and into New Jersey, as the limestones between the Olenellus zone and the Trenton zone represent the intervals of the middle and upper Cambrian and the lower Ordovician, or the Calciferous and Chazy zones, of the New York section. The working out of

*Geol. Surv. New Jersey; Ann. Rep. State Geologist, 1890, pp. 31, 43, 49.

the details of this section in southeastern Pennsylvania is an interesting problem, left for solution to some geologist who has the necessary paleontologic training and who will not be discouraged by the prospect of a good deal of hard work before the desired result can be obtained.

The problem of where to draw the line in this series of limestones, on a geological map, between the Cambrian and Ordovician, is one that will seriously embarrass the geologist, but I anticipate that either lithologic or paleontologic characters will be discovered by which the two groups can be differentiated. If not, the limestones must be colored as one lithologic unit or formation and the approximate line of demarkation between the Cambrian and Ordovician indicated in the columnar section accompanying the legend of the map.

Art. V. — *The Internal Work of the Wind;** by S. P. Langley. (With Plates I–V).

Part I.—*Introductory.*

It has long been observed that certain species of birds maintain themselves indefinitely in the air by "soaring," without any flapping of the wing, or any motion other than a slight rocking of the body; and this, although the body in question is many hundred times denser than the air in which it seems to float with an undulating movement, as on the waves of an invisible stream.

No satisfactory mechanical explanation of this anomaly has been given, and none would be offered in this connection by the writer, were he not satisfied that it involves much more than an ornithological problem, and that it points to novel conclusions of mechanical and utilitarian importance. They are paradoxical at first sight, since they imply that under certain specified conditions, very heavy bodies entirely detached from the earth, immersed in, and free to move in, the air, can be sustained there indefinitely, without any expenditure of energy from within.

These bodies may be entirely of mechanical construction, as will be seen later, but for the present we will continue to consider the character of the invisible support of the soaring bird, and to study its motions, though only as a pregnant

* A paper read (by title only) to the National Academy of Sciences, in April, 1893, and subsequently (in full) at the Aeronautical Congress, at Chicago, in August, 1893.

instance offered by nature to show that a rational solution of the mechanical problem is possible.

Recurring, then, to the illustration just referred to, we may observe that the flow of an ordinary river would afford no explanation of the fact that nearly inert creatures, while free to move, although greatly denser than the fluid, yet float upon it; which is what we actually behold in the ærial stream, since the writer, like others, has satisfied himself by repeated observation, that the soaring vultures and other birds, appear as if sustained by some invisible support, in the stream of air, sometimes for at least a considerable fraction of an hour. It is frequently suggested by those who know these facts only from books, that there must be some quivering of the wings, so rapid as to escape observation. Those who do know them from observation, are aware that it is absolutely certain that nothing of the kind takes place, and that the birds sustain themselves on pinions which are quite rigid and motionless, except for a rocking or balancing movement involving little energy.

The writer desires to acknowledge his indebtedness to that most conscientious observer, M. Mouillard,* who has described these actions of the soaring-birds with incomparable vividness and minuteness, and who asserts that they under certain circumstances, without flapping their wings, rise and actually advance against the wind.

To the writer, who has himself been attracted from his earliest years to the mystery which has surrounded this action of the soaring bird, it has been a subject of continual surprise that it has attracted so little attention from physicists. That nearly inert bodies, weighing from 5 to 10, and even more, pounds, and many hundred times denser than the air, should be visibly suspended in it above our heads, sometimes for hours at a time, and without falling,—this, it might seem, is, without misuse of language, to be called a physical miracle; and yet the fact that those whose province it is to investigate nature, have hitherto seldom thought it deserving attention, is perhaps the greater wonder.

This indifference may be in some measure explained by the fact that the largest and best soarers are of the vulture kind, and that their most striking evolutions are not to be seen in those regions of the Northern Temperate Zone where the majority of those whose training fits them to study the subject, are found. Even in Washington, however, where the writer at present resides, scores of great birds may be seen at times in the air together, gliding with and against the wind, and ascending higher at pleasure, on nearly motionless wings. "Those who have not seen it," says M. Mouillard, "when they

* L. P. Mouillard, "L'Empire de l'Air," Paris. G. Masson.

are told of this ascension without the expenditure of energy, are always ready to say, 'but there must have been movements, though you did not see them;'" "and in fact," he adds, "the casual witness of a single instance, himself, on reflection, feels almost a doubt as to the evidence of his senses, when they testify to things so extraordinary."

Quite agreeing with this, the writer will not attempt any general description of his own observations, but as an illustration of what can sometimes be seen, will give a single one, to whose exactness he can personally witness. The common "Turkey Buzzard" (*Cathartes aura*), is so plenty around the environs of Washington that there is rarely a time when some of them may not be seen in the sky, gliding in curves over some attractive point, or, more rarely, moving in nearly straight lines on rigid wings, if there be a moderate wind. On the only occasion when the motion of one near at hand could be studied in a very high wind, the author was crossing the long "Aqueduct Bridge" over the Potomac, in an unusually violent November gale, the velocity of the wind being probably over 35 miles an hour. About one-third of the distance from the right bank of the river, and immediately over the right parapet of the bridge, at a height of not over 20 yards, was one of these buzzards, which, for some object which was not evident, chose to keep over this spot, where the gale, undisturbed by any surface irregularities, swept directly up the river with unchecked violence. In this ærial torrent, and apparently indifferent to it, the bird hung, gliding in the usual manner of its species, round and round, in a small oval curve, whose major axis (which seemed toward the wind), was not longer than twice its height from the water. The bird was therefore at all times in close view. It swung around repeatedly, rising and falling slightly in its course, while keeping, as a whole, on one level, and over the same place, moving with a slight swaying, both in front and lateral direction, but in such an effortless way as suggested a lazy yielding of itself to the rocking of some invisible wave.

It may be asserted that there was not only no flap of the wing, but not the quiver of a wing feather visible to the closest scrutiny, during the considerable time the bird was under observation, and during which the gale continued. A record of this time was not kept, but it at any rate lasted until the writer, chilled by the cold blast, gave up watching and moved away, leaving the bird still floating about, at the same height in the torrent of air, in nearly the same circle, and with the same aspect of indolent repose.

If the wind is such a body as it is commonly supposed to be, it is absolutely impossible that this sustention could have

taken place in a horizontal current any more than in a calm, and yet that the ability to soar is, in some way, connected with the presence of the wind, became to the writer, as certain as any fact of observation could be, and at first the difficulty of reconciling such facts (to him undoubted) with accepted laws of motion, seemed quite insuperable.

Light came to him through one of those accidents which are commonly found to occur when the mind is intent on a particular subject, and looking everywhere for a clue to its solution.

In 1887, while engaged with the "whirling-table" in the open air at the Allegheny Observatory, he had chosen a quiet afternoon for certain experiments, but in the absence of the entire calm which is almost never realized, had placed one of the very small and light anemometers made for hospital use, in the open air, with the object of determining and allowing for the velocity of what feeble breeze existed. His attention was called to the extreme irregularity of this register, and he assumed at first that the day was more unfavorable than he had supposed. Subsequent observations, however, showed that when the anemometer was sufficiently light and devoid of inertia, the register always showed great irregularity, especially when its movements were noted, not from minute to minute, but from second to second.

His attention once aroused to these anomalies, he was led to reflect upon their extraordinary importance in a possible mechanical application. He then designed certain special apparatus hereafter described, and made observations with it which showed that "wind" in general, was not what it is commonly assumed to be, that is,—air put in motion with an approximately uniform velocity in the same strata; but that considered in the narrowest practicable sections, wind was always not only not approximately uniform, but variable and irregular in its movements beyond anything which had been anticipated, so that it seemed probable that the very smallest part observable, could not be treated as approximately homogeneous, but that even here, there was an internal motion to be considered, distinct both from that of the whole body, and from its immediate surroundings. It seemed to the writer to follow as a necessary consequence, that there might be a potentiality of what may be called "internal work"* in the wind.

* Since the term "internal work" is often used in thermo-dynamics to signify molecular action, it may be well to observe that it here refers not to molecular movements, but to pulsations of sensible magnitude, always existing in the wind, as will be shown later, and whose extent and extraordinary possible mechanical importance it is the object of this research to illustrate. The term is so significant of the author's meaning, that he permits himself the use of it here, in spite of the possible ambiguity.

On further study, it seemed to him that this internal work might conceivably be so utilized as to furnish a power which should not only keep an inert body from falling, but cause it to rise, and that while this power was the probable cause of the action of the soaring bird, it might be possible through its means to cause any suitably disposed body, animate or inanimate, wholly immersed in the wind, and wholly free to move, to advance against the direction of the wind itself. By this it is not meant that the writer then devised means for doing this, but that he then attained the conviction both that such an action involved no contradiction of the laws of motion, and that it was mechanically possible (however difficult it might be to to realize the exact mechanism, by which this might be accomplished).

It will be observed that in what has preceded, it is intimated that the difficulties in the way of regarding this even in the light of a theoretical possibility, may have proceeded, with others as with the writer, not from erroneous reasoning, but from an error, in the premises, entering insidiously in the form of the tacit assumption made by nearly all writers, that the word "wind" means something so simple, so readily intelligible, and so commonly understood, as to require no special definition; while, nevertheless, the observations which are presently to be given, show that it is, on the contrary, to be considered as a generic name for a series of infinitely complex and little known phenomena.

Without determining here whether any mechanism can be actually devised which shall draw from the wind the power to cause a body wholly immersed in it to go against the wind, the reader's consideration is now first invited to the evidence that there is no contradiction to the known laws of motion, and at any rate no theoretical impossibility in the conception of such a mechanism, if it is admitted that the wind is not what it has been ordinarily taken to be, but what the following observations show that it is.

What immediately follows is an account of evidence of the complex nature of the "wind," of its internal movements, of the resulting potentiality of this internal work, and of attempts which the writer has made to determine quantitatively its amount by the use of special apparatus, recording the changes which go on (so to speak) *within* the wind in very brief intervals. These results may, it is hoped, be of interest to meteorologists, but they are given here with special reference to their important bearing on the future of what the writer has ventured to call the science of Aerodromics.*

* From *ἀεροδρομεω*, to traverse the air; *ἀεροδρομος*, an air-runner.

The observations which are first given were made in 1887 at Allegheny and are supplemented by others made at Washington in the present year.*

What has just been said about their possible importance will perhaps seem justified, if it is remarked (in anticipation of what follows later) that the result of the present discussion implies not only the theoretical, but the mechanical possibility, that a heavy body wholly immersed in the air and sustained by it, may without the ordinary use of wind, or sail, or steam, and without the expenditure of any power except such as may be derived from the ordinary winds, make an aerial voyage in any direction, whose length is only limited by the occurrence of a calm. A ship is able to go against a head wind by the force of that wind, owing to the fact that it is partly immersed in the water which reacts on the keel, but it is here asserted, that (contrary to usual opinion and in opposition to what at first may seem the teachings of physical science) it is not impossible that a heavy and nearly inert body, *wholly* immersed in the air can be made to do this.

The observations on which the writer's belief in this mechanical possibility are founded, will now be given.

Part II.—*Experiments with the use of special apparatus.*

In the ordinary uses of the anemometer,—(let us suppose it to be a Robinson's anemometer, for illustration,—the registry is seldom taken as often as once a minute; thus, in the ordinary practice of the United States Weather Bureau, the registration is made at the completion of the passage of each mile of wind. If there be very rapid fluctuations of the wind, it is obviously desirable in order to detect them, to observe the instrument at very brief intervals, *e. g.* at least every second, instead of every minute or every hour, and it is equally obvious that in order to take up and indicate the changes which occur in these brief intervals, the instrument should have as little inertia as possible, its momentum tending to falsify the facts, by rendering the record more uniform than would otherwise be the case.

*It will be noticed that the fact of observation here is not so much the movement of currents, such as the writer has since learned was suggested by Lord Rayleigh so long ago as 1883, still less of the movement of distinct currents at a considerable distance above the earth's surface, but of what must be rather called the effect of the irregularities and pulsations of any ordinary wind, within the immediate field of examination, however narrow.

See he instructive article by Lord Rayleigh in Nature. April 5, 1883. Lord Rayleigh remarks that continued soaring implies "(1) that the course is not horizontal, (2) that the wind is not horizontal, or (3) that the wind is not uniform." "It is probable," he says, "that the truth is usually represented by (1) or (2): but the question I wish to raise is whether the cause suggested by (3) may not sometimes come into operation."

In 1887 I made use of the only apparatus at command,—an ordinary small Robinson's anemometer, having cups 3 inches (7·5^{cm}) in diameter, the centre of the cups being $6\frac{3}{4}$ inches ($16\frac{3}{4}^{cm}$) from the centre of rotation. This was placed at the top of a mast 53 feet (16·2 metres) in height, which was planted in the grounds of the Allegheny Observatory, on the flat summit of a hill which rises nearly 400 feet (122· metres) above the valley of the Ohio river. It was, accordingly, in a situation exceptionally free from those irregularities of the wind which are introduced by the presence of trees and of houses, or of inequalities of surface.

Every twenty-fifth revolution of the cups was registered by closing an electric circuit, and the registry was made on the chronograph of the Observatory by a suitable electric connection, and these chronograph sheets were measured and the results tabulated. A portion of the record obtained on July 16, 1887, is given on Plate I, the abscissae representing time, and the ordinates wind velocities. The observed points represent the wind's velocities as computed from the intervals between each successive electrical contact, as measured on the chronograph sheets, and for convenience in following the succession of observed points, they are here joined by straight lines, though it is hardly necessary to remark that the change in velocity is in fact, though quite sharp, yet not in general discontinuous, and the straight lines here used for convenience do not imply that the rate of change of velocity is uniform.

The wind velocities during this period of observation ranged from about 10 to 25 miles an hour, and the frequency of measurement was every 7 to 17 seconds. If, on the one hand, owing to the weight and inertia of the anemometer, this is far from doing justice to the actual irregularities of the wind; on the other, it equally shows that the wind was far from being a body of even approximate uniformity of motion, and that even when considered in quite small sections, the motion was found to be irregular almost beyond conception,—certainly beyond anticipation; for this record is not selected to represent an extraordinary breeze, but the normal movement of an ordinary one.

By an application of these facts, to be presented later, I then reached by these experiments, the conclusion that it was theoretically possible to cause a heavy body wholly immersed in the wind to be driven in the opposite direction, *e. g.* to move east while the wind was blowing west, without the use of any power other than that which the wind itself furnished, and this even by the use of plane surfaces, and without taking advantage of the more advantageous properties of curved ones.

This power, I further already believed myself warranted by these experiments in saying, could be obtained by the movements of the air in the horizontal plane alone, even without the utilization of currents having an upward trend. But I was obliged to turn to other occupations, and did not resume these interesting observations until the year 1893.

Although the anemometer used at Allegheny served to illustrate the essential fact of the rapid and continuous fluctuations of even the ordinary and comparatively uniform wind, yet owing to the inertia of the arms and cups, which tended to equalize the rate (the moment of inertia was approximately 40,000 gr. cm^2) and to the fact that the record was only made at every twenty-fifth revolution, the internal changes in the horizontal component of the wind's motion, thus representing its potential work, were not adequately recorded.

In January, 1893, I resumed these observations at Washington with apparatus with which I sought to remedy these defects, using as a station the roof of the north tower of the Smithsonian Institution building, the top of the parapet being 142 feet (43·3 meters) above the ground, and the anemometers, which were located above the parapet being 153 feet (46·7 meters) above the ground. I placed them in charge of Mr. George E. Curtis, with instructions to take observations under the conditions of light, moderate and high winds. The apparatus used was, first, a Weather Bureau Robinson anemometer of standard size, with aluminum cups. Diameter to center of cups 34^{cm}; diameter of cups $10{\cdot}16^{cm}$; weight of arms and cups 241 grams; approximate moment of inertia 40,710 gr. cm^2.

A second instrument was a very light anemometer, having paper cups, of standard pattern and diameter, the weight of arms and cups being only 74 grams, and its moment of inertia 8,604 gr. cm^2.

With this instrument, a number of observations were taken, when it was lost by being blown away in a gale. It was succeeded in its use by one of my own construction, which was considerably lighter. This was also blown away. I afterward employed one of the same size as the standard pattern, weighing 48 grams, having a moment of inertia of 11,940 gr. cm^2, and finally, I constructed one of one-half the diameter of the standard pattern, employing cones instead of hemispheres, weighing 5 grams, and having a moment of inertia of but 300 gr. cm^2.

In the especially light instruments, the electric record was made at every half revolution, on an ordinary astronomical chronograph, placed upon the floor of the tower, connected with the anemometer by an electric circuit. Observations

were made on Jan. 14, 1893, during a light wind having a velocity of from 9 to 17 miles an hour; on January 25 and 26 during a moderate wind, having a velocity of from 16 to 28 miles an hour; and on February 4 and 7, during a moderate and high wind ranging from 14 to 36 miles an hour. Portions of these observations are given on Plates II, III and IV. A short portion of the record obtained with the standard Weather Bureau anemometer during a high northwest wind is given on Plate V.

A prominent feature presented by these diagrams is that the higher the absolute velocity of the wind, the greater the relative fluctuations which occur in it. In a high wind the air moves in a tumultuous mass, the velocity being at one moment perhaps 40 miles an hour, then diminishing to an almost instantaneous calm, and then resuming.*

The fact that an absolute local calm can momentarily occur during the prevalence of a high wind, was vividly impressed upon me during the observations of February 4, when chancing to look up to the light anemometer, which was revolving so rapidly that the cups were not separately distinguishable, I saw them completely stop for an instant, and then resume their previous high speed of rotation, the whole within the fraction of a second. This confirmed the suspicion that the chronographic record, even of a specially light anemometer, but at most imperfectly notes the sharpness of these internal changes. Since the measured interval between two electric contacts is the datum for computing the velocity, an instantaneous stoppage, such as I accidentally saw, will appear on the record simply as a slowing of the wind, and such very significant facts as that just noted, will be necessarily slurred over, even by the most sensitive apparatus of this kind.

However, the more frequent the contacts, the more nearly an exact record of the fluctuations may be measured, and I have, as I have stated, provided that they should be made at every half revolution of the anemometer, that is, as a rule, several times a second.†

* An example of a very rapid change may be seen on Plate IV, at 12.23 P. M.

† Here we may note the error of the common assumption that the ordinary anemometer, however heavy, will, if frictionless, correctly measure the velocity of the wind, for the existence of "vis inertiæ" it is now seen, is not indifferent, but plays a most important part where the velocity suffers such great and frequent changes as we here see it does, and where the rate at which this inertia is overcome, and this velocity changed, is plainly a function of the density of the fluid, which density we also see reason to suppose itself varies incessantly, and with great rapidity. Though it is probable that no form of barometer in use does justice to the degree of change of this density, owing to this rapidity, we cannot nevertheless, suppose it to exceed certain limits, and we may treat the present records, made with an anemometer of such exceptional lightness, as being comparatively unaffected by these changes in density, though they exist.

I now invite the reader's attention to the actual records of rapid changes that take place in the wind's velocity, selecting as an illustration, the first 5½ minutes of the diagram plotted on Plate III.

The heavy line through points A, B, and C, represents the ordinary record of the wind's velocity as obtained from a standard Weather Bureau anemometer during the observations recording the passage of two miles of wind. The velocity, which was at the beginning of the interval considered, nearly 23 miles an hour, fell during the course of the first mile to a little over 20 miles an hour. This is the ordinary anemometric record of the wind at such elevations as this (47 meters) above the earth's surface, where it is free from the immediate vicinity of disturbing irregularities, and where it is popularly supposed to move with occasional variation in direction, as the weather-cock indeed indicates, but with such nearly uniform movement that its rate of advance is, during any such brief time as two or three minutes, under ordinary circumstances, approximately uniform. This then may be called the "wind," that is, the conventional "wind" of treatises upon aerodynamics, where its aspect as a practically continuous flow, is alone considered. When, however, we turn to the record made with the specially light anemometer, at every second, of this same wind, we find an entirely different state of things. The wind, starting with the velocity of 23 miles an hour, at $12^{hrs}\ 10^{mins}\ 18^{secs}$ rose within 10 seconds to a velocity of 33 miles an hour, and within 10 seconds more fell to its initial speed. It then arose within 30 seconds to a velocity of 36 miles an hour, and so on, with alternate risings and fallings, at one time actually stopping; and, as the reader may easily observe, passing through 18 notable maxima and as many notable minima, the average interval from a maximum to a minimum being a little over 10 seconds, and the average change of velocity in this time being about 10 miles an hour. In the lower left hand corner of Plate III, is given a conventional representation of these fluctuations in which this average period and amplitude is used as a type. The above are facts, the counterpart of which may be noted by any one adopting the means the writer has employed. It is hardly necessary to observe, that almost innumerable minor maxima and minima presented themselves, which the drawing cannot depict.

In order to insure clearness of perception, the reader will bear in mind the diagram does not represent the velocities which obtained coincidently, along the length of two miles of wind represented, nor the changes in velocity experienced by a single moving particle during the interval, but that it is a picture of the velocities which were in this wind at the succes-

sive instants of its passing the fixed anemometer, which velocities, indeed, were probably nearly the same for a few seconds before and after registry, but which incessantly passed into—and were replaced by—others, in a continuous flow of change. But although the observations do not show the actual changes of velocity which any given particle experiences in any assigned interval, these fluctuations cannot be materially different in character from those which are observed at a fixed point, and are shown in the diagram. It may perhaps still further aid us in fixing our ideas, to consider two material particles as starting at the same time over this two mile course; the one moving with the uniform velocity of 22·6 miles an hour (33 feet per second), which is the average velocity of the wind as observed for the interval between $12^{hrs}\ 10^{mins}\ 18^{secs}$—and $12^{hrs}\ 15^{mins}\ 45^{secs}$ on February 4; the other, during the same interval, having the continuously changing velocities actually indicated by the light anemometer as shown on Plate III. Their positions at any time may, if desired, be conveniently represented in a diagram where the abscissa of any point represents the elapsed time in seconds, and the ordinates show the distance in feet, of the material particle from the starting point. The path of the first particle will thus be represented by a straight line, while the path of the second particle will be an irregularly curved line, at one time above, and at another time below the mean straight line just described, but terminating in coincidence with it at the end of the interval. If, now, all the particles in two miles of wind were simultaneously accelerated and retarded in the same way as this second particle, that is, if the wind were an inelastic fluid, and moved like a solid cylinder, the velocities recorded by the anemometer would be identical with those that obtained along the whole region specified. But the actual circumstance must evidently be far different from this, since the air is an elastic, and nearly perfect fluid, subject to condensation and rarefaction. Hence the successive velocities of any given particle (which are in reality the resultant of incessant changes in all directions), must be conceived as evanescent, taking on something like the sequence recorded by these curves, a very brief time before this air reached the anemometer, and losing it as soon after.

It has not been my purpose in this paper to enter upon any inquiry as to the cause of this non-homogeneity of the wind. The irregularities of the surface topography (including buildings, and every other surface obstruction) are commonly adduced as a sufficient explanation of the chief irregularities of the surface wind; yet I believe that at a considerable distance above the earth's surface (e. g. one mile), the wind may not even be approximately homogeneous, nor have an even

flow; for, while if we consider air as an absolutely elastic and frictionless fluid, any motion impressed upon it would be preserved forever, and the actual irregularities of the wind would be the results of changes made at any past time, however remote; so long as we admit that the wind without being absolutely elastic and frictionless, is nearly so, it seems to me that we may consider that the incessant alternations which it here appears make the "wind," are due to past impluses and changes which are preserved in it, and which die away with very considerable slowness. If this be the case, it is less difficult to see how even in the upper air, and at every altitude, we might expect to find local variations, or pulsations, not unlike those which we certainly observe at minor altitudes above the ground.*

Part III.—*Application.*

Of these irregular movements of the wind, which take place up, down, and on every side, and are accompanied of necessity by equally complex condensations and expansions, it will be observed that only a small portion, namely, those which occur in a narrow current whose direction is horizontal and sensibly linear, and whose width is only the diameter of the anemometer, can be noted by the instruments I have here described, and whose records alone are represented in the diagram. However complex the movement may appear as shown by the diagram, it is then far less so than the reality, and it is probable indeed, that anything like a fairly complete graphical presentation of the case is impossible.

I think that on considering these striking curves (Plates I, II, III, IV and V) we shall not find it difficult to admit, at least as an abstract conception, that there is no necessary violation of the principle of the conservation of energy, implied in the admission that a body wholly immersed in and moving with such a wind, may derive from it a force which may be utilized in *lifting* the body, in a way in which a body immersed in the "wind" of our ordinary conception could not be lifted, and if we admit that the body may be lifted, it follows obviously that it may descend under the action of gravity from the elevated position, on a sloping path, to some distance in a direction opposed to that of the wind which lifted it, though it is not obvious what this distance is.

We may admit all this, because we now see (I repeat) that the apparent violation of law arises from a tacit assumption which we in common with all others, may have made, that the

* In this connection, reference may be made to the notable investigations of Helmholtz, on Atmospheric Movements, *Sitzungsberichte*, Berlin, 1888–1889.

wind is an approximately homogeneously moving body, because moving as a whole in one direction. It is, on the contrary, *always*, as we see here, filled (even if we consider only movments in some one horizontal plane), with amazingly complex motions, some of which if not in direct opposition to the main movement, are relatively so, that is, are slower, while others are faster than this main movement, so that a portion is always opposed to it.

From this, then, we may now at least see that it is plainly within the capacity of an intelligence like that suggested by Maxwell, and which Lord Kelvin has called the "Sorting Demon," to pick out from the internal motions, those whose direction is opposed to the main current, and to omit those which are not so, and thus *without the expenditure of energy* to construct a force which will act against the main current itself.

But we may go materially further, and not only admit that it is not necessary to invoke here, as Maxwell has done in the case of thermo-dynamics, a being having power and rapidity of action far above ours, but that in actual fact, a being of a lower order than ourselves, guided only by instinct, may so utilize these internal motions.

We might not indeed have conceived this possible, were it not that nature has already, to a large extent, exhibited it before our eyes in the soaring bird* which sustains itself endlessly in the air with nearly motionless wings, for without this evidence of the possibility of an action which now ceases to approach the inconceivable, we are not likely, even if admitting its theoretical possibility to have thought the mechanical solution of this problem possible.

* "When the condors in a flock are wheeling round and round any spot, their flight is beautiful. Except when rising from the ground, I do not recollect ever having seen one of these birds flap its wings. Near Lima, I watched several for nearly half an hour without once taking off my eyes. They moved in large curves sweeping in circles, descending and ascending without once flapping. As they glided close over my head, I intently watched, from an oblique position, the outlines of the separate and terminal feathers of the wing; and if there had been the least vibratory movement these would have blended together, but they were seen distinct against the blue sky. The head and neck were moved frequently and apparently with force, and it appeared as if the extended wings formed the fulcrum on which the movements of the neck, body, and tail acted. If the bird wished to descend, the wings for a moment collapsed; and then when again expanded with an altered inclination the momentum gained by the rapid descent, seemed to urge the bird upwards, with the even and steady movement of a paper kite. In the case of any bird *soaring*, its motion must be sufficiently rapid so that the action of the inclined surface of its body on the atmosphere may counterbalance its gravity. The force to keep up the momentum of a body moving in a horizontal plane in that fluid (in which there is so little friction) cannot be great, and this force is all that is wanted. The movement of the neck and body of the condor, we must suppose is sufficient for this. However this may be, it is truly wonderful and beautiful to see so great a bird, hour after hour, without any apparent exertion, wheeling and gliding over mountain and river."—Darwin's *Journal of the Various Countries Visited by H. M. S. Beagle*, pp. 223–224.

But although to show how this physical miracle of nature is to be imitated, completely and in detail, may be found to transcend any power of analysis, I hope to show that this may be possible without invoking the asserted power of "aspiration" relative to curved surfaces, or the trend of upward currents, and even to indicate the probability that the mechanical solution of this problem may not be beyond human skill.

To this conclusion we are invited by the following consideration, among others.

We will presently examine the means of utilizing this potentiality of internal work in order to cause an inert body wholly unrestricted in its motion and wholly immersed in the current, to *rise;* but first let us consider such a body (a plane) whose movement is restricted to a horizontal direction, but which is free to move between frictionless vertical guides. Let it be inclined upward at a small angle towards a horizontal wind so that only the vertical component of the pressure of the wind on the plane will affect its motion. If the velocity of the wind be sufficient, the vertical component of pressure will equal or exceed the weight of the plane, and in the latter case, the plane will rise indefinitely.

Thus, to take a concrete example, if the plane be a rectangle whose length is six times its width, having an area of 2·3 square feet to the pound, and be inclined at an angle of 7°, and if the wind have a velocity of 36 feet per second, experiment shows that the upward pressure will exceed the weight of the plane, and the plane will rise, if between vertical nearly frictionless guides, at an increasing rate, until it has a velocity of 2·52 feet per second,* at which speed the weight and upward pressure are in equilibrium. Hence there are no unbalanced forces acting, and the plane will have attained a state of uniform motion.

For a wind that blows during 10 seconds, the plane will therefore rise about 25 feet. At the beginning of the motion, the inertia of the plane makes the rate of rise less than the uniform rate, but at the end of 10 seconds, the inertia will cause the plane to ascend a short distance after the wind has ceased, so that the deficit at the beginning will be counterbalanced by the excess at the end of the assigned interval.

We have just been speaking of a material heavy plane permanently sustained in vertical guides, which are essential to its continuous ascent in a uniform wind, but such a plane will be lifted and sustained *momentarily* even if there be no vertical guides, or in the case of a kite, even if there be no cord to retain it, the inertia of the body supplying for a brief period,

* See "Experiments in Aerodynamics," by S. P. Langley, *Smithsonian Contributions to Knowledge*, 1891.

the office of the guides or of the cord. If suitably disposed, it will, as the writer has elsewhere shown, under the resistance to a horizontal wind, imposed only by its inertia, commence to move, not in the direction of the wind, but nearly vertically. Presently however, as we recognize, this inertia must be overcome, and as the inclined plane takes up more and more the motion of the wind, the lifting effect must grow less and less (that is to say, if the wind be the approximately homogeneous current it is commonly treated as being), and finally ceasing altogether, the plane must ultimately fall. If, however a counter-current is supposed to meet this inclined plane, before the effect of its inertia is exhausted, and consequently before it ceases to rise, we have only to suppose the plane to be rotated through 180° about a vertical axis, without any other call for the expenditure of energy, to see that it will now be lifted still higher, owing to the fact that its inertia now reappears as an active factor. The annexed sketch (fig. 1) shows a typical representation of what might be supposed to happen with a model inclined plane freely suspended in the air, and endowed with the power of rotating about a vertical axis so as to change the aspect of its constant inclination, which need involve no (theoretical) expenditure of energy, even although the plane possess inertia. We see that this plate would rise indefinitely by the action of the wind in alternate *directions*.

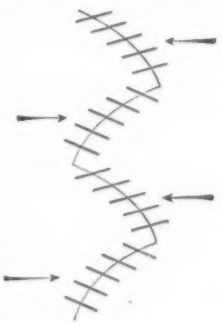

Fig. 1.

The disposition of the wind which is here supposed to cause the plane to rise, appears at first sight an impossible one, but we shall next make the important observation that it becomes virtually possible by a method which we shall now point out, and which leads to a practicable one which we may actually employ.

Figure 2 shows the wind blowing in one constant direction, but alternately at two widely varying velocities or rather (in the extreme case supposed in illustration), where one of the velocities is negligibly small, and where successive pulsations in the same direction are separated by intervals of calm.

A frequent alternation of velocities, united with constancy of absolute direction, has previously been shown here to be the ordinary condition of the wind's motion; but attention is now particularly called to the fact that while these unequal velocities may be in the same direction as regards the surface of the earth yet as regards the *mean* motion of the wind, they

are in opposite directions, and will produce on a plane, whose inertia enables it to sustain a sensibly uniform motion with the mean velocity of this variable wind, the same lifting effect as if these same alternating winds were in absolutely opposed directions, provided that the (constant inclination of the plane alternates in its aspect to correspond with the changes in the wind.

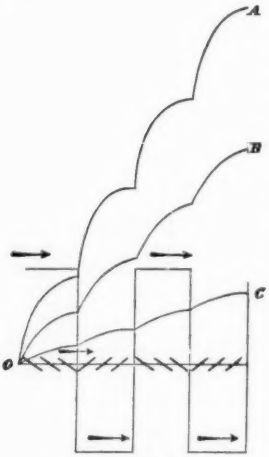

Fig. 2.

It may aid in clearness of conception, if we imagine a set of fixed co-ordinates X Y Z passing through 0, and a set of movable co-ordinates $x\ y\ z$, moving with the velocity and the direction of the mean wind. If the moving body is referred to these first only, it is evidently subject to pulsations which take place in the same directions on the axis of X, but it must be also evident that if referred to the second or movable co-ordinates, these same pulsations may be and are, in opposite directions. This, then, is the case we have just considered, and if we suppose the plane to change the aspect* of its (constant) inclination as the direction of the pulsations changes, it is evident that there must be a gain in altitude with every pulsation, while the plane advances horizontally with the velocity of the mean wind.

During the period of maximum wind velocity, when the wind is moving faster than the plane, the rear edge of the latter must elevated. During the period of minimum velocity, when the plane, owing to its inertia is moving faster than the wind, the front edge of the plane must be elevated. Thus the vertical component of the wind pressure as it strikes the oblique plane, tends in both cases, to give it a vertical upward thrust. So long as this thrust is in excess of the weight to be lifted, the plane will rise. The rate of rise will be greatest at the beginning of each period, when the relative velocity is greatest, and will diminish as the resistance produces "drift;" *i. e.* diminishes relative velocity. The curved line O B in the vignette, represents a typical path of the plane under these conditions.

* We do not for the moment consider how this change of aspect is to be mechanically effected; we only at present call attention to the fact that it involves, in theory, no expenditures of energy.

It follows from the diagram (fig. 1) that other things being equal the more frequent the wind's pulsations, the greater will be the rise of the plane, for since during each period of steady wind, the rate of rise diminishes, the more rapid the pulsations, the nearer the mean rate of rise will be to the initial rate. The requisite frequency of pulsations is also related to the inertia of the plane, as the less the inertia, the more frequent must be the pulsations in order that the plane shall not lose its relative velocity.

It is obvious that there is a limit or weight which cannot be exceeded if the body is to be sustained by any such fluctuations of velocity as can be actually experienced. Above this limit of weight the body will sink. Below this limit the lighter the body is the higher it will be carried, but with increasing variability of speed. That body, then, which has the greatest weight per unit of surface, will soar with the greatest steadiness, if it soar at all, not on account of this weight *per se*, but because the weight is an index of its inertia.

The reader who will compare the results of experiments made with any artificial flying models, like those of Penaud, with the weights of the soaring birds as given in the tables by M. Mouillard, or other authentic sources, cannot fail to be struck with the great weight in proportion to wing surface which nature has given to the soaring bird, compared with any which man has yet been able to imitate in his models.

This fact of the weight of the soaring bird in proportion to its area, has been again and again noted, and it has been frequently remarked that without weight the bird could not soar, by writers who felt that they could very safely make such a paradoxical statement, in view of the evidence nature everywhere gave that this weight was indeed in some way necessary to rising. But these writers have not shown, so far as I remember, how this necessity arises, and this is what I now endeavor to point out.*

It has not here been shown what limit of weight is imposed to the power of an ordinary wind to elevate and sustain, but it seems to me, and I hope that it may seem to the reader, that the evidence that there is *some* weight which the action of the wind is sufficient to permanently sustain under these conditions in a free body, has a demonstrative character, although no quantitative formula is offered at this stage of the investigation. It is obvious that, if this weight is sustainable

*It is perhaps not superfluous to recall here that, according to the researches of Rankine, Froude and others, a body moulded in wave-line curves would, if frictionless, continue to move indefinitely against an opposed wind, in virtue of inertia and once acquired velocity, and also to recall how very small the effect of fluid friction in the air has been shown to be (by the writer in a previous investigation).

at any height, gravity may be utilized to cause the body (which we suppose to be a material plane) to descend on an inclined course, to some distance, even against the wind.

I desire, in this connection to remark that the preceding experiments and deductions showing that a material free plane,* possessing sufficient inertia, may in theory rise indefinitely by the action of an ordinary wind, without the expenditure of work from any internal source (as well as those statements, which follow), when these explanations are once made, have a character of obviousness, which is due to the simplicity of the enunciation, but not, I think, to the familiarity of the explanation, for though attention is beginning to be paid by meteorologists to the rapidity of these wind fluctuations, I am not aware that their effects have been so exhibited, or especially, that they have been presented in this connection, or that the conclusions which follow have been drawn from them.

We have here seen, then, how pulsations of sufficient amplitude and frequency, of the kind which present themselves in nature, may, in theory, furnish energy not only sufficient to sustain, but actually to elevate a heavy body moving in and with the wind at its mean rate.

It is easy to now pass to the practical case which has been already referred to, and which is exemplified in nature, namely, that in which the body (*e. g.* the bird soaring on rigid wings, but having power to change its inclination) uses the elevation thus gained to move against the wind, without expending any sensible amount of its own energy. Here the upward motion is designedly arrested at any convenient stage, *e. g.* at each alternate pulsation of the wind, and the height attained is utilized so that the action of gravity may carry the body by its descent in a curvilinear path (if necessary) against the wind. It has just been pointed out that if some height has been attained, the theoretical possibility of *some* advance against the wind in so falling, hardly needs demonstration, though it may not unnaturally be supposed that the relative advance so gained must be insignificant, compared with the distance traveled by the mean wind while the body was being elevated, so that on the whole the body is carried by the wind, further than it advances against it.

This however, probably need not be in fact the case, there being, as it appears to me from experiment and from deduction,

* I use the word "plane," but include in the statement all suitable modifications of a curved surface.

I desire to recall attention to the paragraph in "Experiments in Aerodynamics" in which I caution the reader against supposing that by investigating plane surfaces, I imply that they are the best form of surface for flight; and I repeat here that, as a matter of fact, I do not believe them to be so. I have selected the plane simply as the best form for preliminary experiment.

every reason to believe that under suitable conditions, the advance may be greater than the recession, or that the body falling under the action of gravity along a suitable path, may return against the wind, not only from Z to O, the point of departure, but further as is here shown.

I repeat however, that I am not at the moment undertaking to demonstrate how the action is mechanically realizable in actual practice, but only that it is possible. It is for this purpose, and to understand more exactly that it can be effected, not only by the process indicated, in the second illustration (fig. 2) but by another and probably more usual one (and nature has still others at command), that I have considered another treatment of the same conditions, of wind-pulsations always moving in the same horizontal direction, but for brief periods, interrupted by equal intervals of calm. In this third illustration (fig. 3) we suppose the body to use the height gained in each pulsation, to enable it to descend after each such pulsation and advance against the direction of the wind.

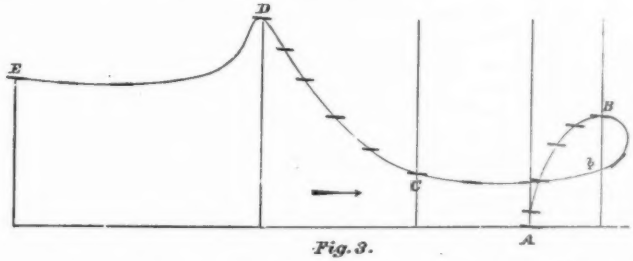

Fig. 3.

The portion A B of the curve represents the path of the plane surface from a state of rest at A, where it has a small upward inclination toward the wind. If a horizontal wind blow upon it in the direction of the arrow, the first movement of the plane will not be in the direction of the wind, but as is abundantly demonstrated by the writer in "Experiments in Aerodynamics" it will rise in a nearly vertical direction, if the angle be small. The wind, continuing to blow in the same direction, at the end of a certain time, the plane, which has risen (owing to its inertia, and in spite of its weight), to the successive positions shown, is taking up more and more of the horizontal velocity of the wind, and consequently opposing less resistance to it, and therefore moving more and more laterally, and rising less and less, at every successive instant.

If the wind continued indefinitely, the plane would ultimately take up its velocity, and finally, of course, fall, when this inertia ceased to oppose resistance to the wind's advance.

I have supposed, however, the wind pulsation to cease at the end of a certain brief period, and, to fix our ideas, let us suppose this period to be five seconds. At this moment the period of calms begin, and now let the plane, which is supposed to have reached the point B, change its inclination about a horizontal axis to that shown in the diagram, falling at first nearly vertically, with its edge on the line of its descent, so as to acquire speed, and this speed, acquired, by constantly changing its angle, glide down the curve B *b* C, so that the plane shall be tangential to it at every point of its descending advance. At the end of five seconds of calm it has reached the position C, near the lowest point of its descent, which there is no contradiction to known mechanical laws in supposing *may* be higher than A, and which in fact, according to the most accurate data the writer can gather, *is* higher, in the case of the above period, and in the case of such an actual plane, as has been experimented upon by him.

Now, having reached C, at the end of the five seconds' calm, if the wind blow in the same direction and velocity as before, it will again elevate the plane on the latter's presenting the proper angle, but this time under more favorable circumstances, for at this time the plane is already in motion in a direction opposed to that of the wind, and is already higher than it was in its original position A. Its course, therefore, will be nearly that along the curve C D, during all which time it maintains the original angle α or one very slightly less. Arrived at D, and at the instant when the calm begins, it falls with varying inclination, to the lowest position E (which may be higher than C), which it attains at the end of the five seconds of calm, then rises again (still nearly at the angle α) to a higher position, and so on; the alternation of directions of motion, at the end of each pulsation, growing less and less sharp, and the path finally taking the character of a sinuous curve. We have here assumed that the plane goes against the wind and rises at the same time, in order to illustrate that this is possible, though either alternative may be employed, and the plane, in theory at least, may maintain on the whole a rapid and nearly horizontal, or a slow and nearly vertical course, or anything between.

It is not meant, either, that the alternations which would be observed in nature are as sharp as those here represented, which are intentionally exaggerated, while in all which has just preceded, by an equally intentional exaggeration of the normal action, the wind-pulsations have been supposed to alternate with absolute calm. This being understood, it is scarcely necessary to point out that if the calm is not absolute, but if there are simply frequent successive winds or pulsations of

wind of considerably differing velocity (such as the anemometer observations show, are realized in nature), that the same general effect will obtain,* though we are not entitled to assume from any demonstration thus far given that the total advance will be necessarily greater than that of the whole distance the mean wind has traveled. It may also be observed that the actual actions of the soaring bird may be and doubtless are, more complex in detail than those of this diagram, while yet in their entirety depending on the principles it sets forth.

The theoretical possibility at least will now it is hoped, be granted, not only of the body's rising indefinitely, or of its descending in the interval of calm to a higher level C, than it rose from at A, but of its advancing against the calm or light wind through a distance B C, greater than that of A B, and so on. The writer however repeats that he has reason to suppose from the data obtained by him, that this is not only a theoretical possibility, but a mechanical probability under the conditions stated, although he does not here offer a quantitative demonstration of the fact, other than by pointing to the movements of the soaring-bird and inviting their reconsideration in the light of the preceding statements.

The bird, by some tactile sensibility to the pressure and direction of the air, is able, in nautical phrase, to "see the wind"† and to time its movements, so that without any reference to its height from the ground, it reaches the lowest portion of its descent near the end of the more rapid wind pulsation; but the writer believes that to cause these adaptive changes in an otherwise inert body, with what might almost be called instinctive readiness and rapidity, does not really demand intelligence or even instinct, but that the future ærodrome may be furnished with a substitute for instinct, in what may perhaps allowably be called, a mechanical brain, which yet need not, in his opinion, be intricate in its character. His reasons for this statement, which is not made lightly, must however be reserved for another time.

It is hardly necessary to point out that the nearly inert body in question may also be a human body, guided both by instinct

* The rotation of the body about a vertical axis so as to change the aspect of the inclination as in the first figure, may be illustrated by the well-known habit of many soaring-birds, of moving in small closed curves or spirals. but it may also be observed, in view of the fact that even in intervals of relative calm during which the body descends, there is always some wind,—that in making the descents, if the body, animate or inanimate, maintain its direct advance, this wind tends to strike on the upper side of the plane or pinion. Mr. G. E. Curtis offers the suggestion that the soaring-bird avoids such a position when possible, and therefore turns at right angles to or with the wind, and that this may be an additional reason for his well known habit of moving in spirals.

† Moullard.

and intelligence, and that there may thus be a sense in which human flight may be possible, although flight depending wholly upon the action of human muscles be forever impossible.

Let me resume the leading points of the present memoir in the statement that it has been shown:

(1) That the wind is not even an approximately uniform moving mass of air, but consists of a succession of very brief pulsations of varying amplitude, and that, relatively to the mean movement of the wind, these are of varying direction.

(2) That it is pointed out that hence there is a potentiality of "internal work" in the wind, and probably of a very great amount.

(3) That it involves no contradiction of known principles to declare that an inclined plane, or suitably curved surface, heavier than the air, freely immersed in, and moving with the velocity of the mean wind, can, if the wind pulsations here described are of sufficient amplitude, and frequency, be sustained or even raised indefinitely without expenditure of internal energy, other than that which is involved in changing the aspect of its inclination at each pulsation.

(4) That since (A) such a surface, having also power to change its inclination, *must* gain energy through falling during the slower, and expend energy by rising during the higher, velocities; and that (B) since it has been shown that there is no contradiction of known mechanical laws in assuming that the surface *may* be sustained or *may* continue to rise indefinitely, the mechanical possibility of some advance against the direction of the wind, follows immediately from this capacity of rising. It is further seen that it is at least possible that this advance against the wind may not only be attained relatively to the position of a body moving with the speed of the mean wind, but absolutely, and with reference to a fixed point in space.

(5) The statement is made that this is not only mechanically possible, but that in the writer's opinion, it is realizable in practice.

Finally, these observations and deductions have, it seems to me an important practical application not only as regards a living creature like the soaring bird but still more as regards a mechanically constructed body, whose specific gravity may probably be many hundred or even many thousand times that of the atmosphere. We may suppose such a body to be supplied with fuel and engines, which would be indispensable to sustain it in a calm, and yet which we now see might be ordinarily left entirely inactive, so that the body could supposably remain in the air and even maintain its motion in any direc-

tion without expending its energy, except as regards the act of changing the inclination or aspect which it presents to the wind, while the wind blew.

The final application of these principles to the art of aerodromics, seems then to be, that while it is not likely that the perfected aerodrome will ever be able to dispense altogether with the ability to rely at intervals on some internal source of power, it will not be indispensable that this aerodrome of the future, shall in order to go any distance—even to circumnavigate the globe without alighting—need to carry a weight of fuel, which would enable it to perform this journey under conditions analogous to those of a steamship, but that the fuel and weight need only be such as to enable it to take care of itself in exceptional moments of calm.

Smithsonian Institution,
Washington, D. C., August, 1893.

Art. VI.—*Post-Glacial Eolian Action in Southern New England;* by J. B. Woodworth.

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The following paper is an account of some detached observations made by the writer while engaged in the survey of the pleistocene deposits of southern New England. The matters touched upon here are too detailed and of too little economic importance to find a place in the short descriptive text accompanying the forthcoming atlas sheets, and are hence published in this form. The field work during which the observations were made extended over the years 1889–1892, and was done under the supervision of Mr. N. S. Shaler, geologist in charge of the Atlantic Coast Division, to whom I am primarily indebted for the privilege of making this independent statement of my observations.

The action of the winds in New England in their geological bearing has been described in several communications. The most significant of these papers is that published in this Journal, in 1886, by Geo. H. Stone.* In this paper, Prof.

* III, vol. xxxi. pp. 133–138. The following is a list of papers by American authors in which carved and polished rock surfaces are described, and generally though not always ascribed to eolian action.

1855. W. P. Blake: Report of a Reconnaissance in Colorado. House Doc. 129, p. 27, and Pacific Ry. Reports, v, pp. 91, 117, 229–232, and this Journal, II, xx, pp. 178–181. 1861. C. H. Hitchcock: Report on Geology of Maine, pp. 266–268; 1870. Gen. Benj. Tilghman: U. S. Patent, No. 108,408; 1875. G. K. Gilbert: U. S. Geog. Surv. 100th Meridian, III, pp. 82–84; 1886 N. S. Shaler: Geology of the Island of Nantucket, U. S. Geol. Survey, Bull. 53; 1886.

Stone for the first time recognized the agency of wind-driven sand in carving rock surfaces. His studies were carried on in the State of Maine, where, he states, "in numerous places, both in Bethel and elsewhere near the mountains, I found bowlders and even small stones which are now being sand-carved by the wind as plainly and incontestably as in Colorado."

The competency of blown-sand to carve rock surfaces.—There can be no question that sands driven by the wind have the power to cut away the surface of rocks on which they impinge. The observations of Blake in San Bernardino Pass, of Gilbert in the Great Basin, of Blanford and Oldway in India, and of numerous observers in the desert regions of the Sahara, the Sinai peninsula, and less arid tracts, point conclusively to the view that by means of this subtle agency the hardest rocks are reduced to detritus and receive a form peculiar to the manner of erosion.

Prestwich, in his Geology (vol. i, page 145), says: "On a land freshly emerged from the sea, both rain and wind had necessarily an infinitely greater effect than now, when it is covered with soil and vegetation. Soft and incoherent strata were then subject to rapid wear by surface waters, and loose materials to easy removal by strong winds, as sands now are on the coast, or on bare tracts inland by the like cause. In the same way volcanic ashes, Diatomaceæ, and seeds are transported for hundreds of miles, and so dispersed over wide areas. Sand, when carried by winds over rocks, is known to wear them smooth, and even to line their surface with scratches and furrows. In some cases, hard rocks, such as granite, quartz, and limestone, have been polished, and quartzite pebbles worn into symmetrical forms by this means. This occurs in countries where certain winds prevail at different times of the year, so that different sides of the pebbles are alternately exposed to the wind-action. In this manner regular pyramidal-shaped pebbles have been fashioned in New Zealand and Egypt."

This quotation sets forth in brief the class of effects which are sparingly observed in the district to which this paper relates. The explanation of the pyramidal-shaped pebbles given by Prestwich differs somewhat from that advanced by Walther, whose account of his observations and the history of opinion in Germany is so good that I venture to quote from his valuable monograph.*

Alex. Winchell: Geological Studies, p. 284; 1886. N. H. Winchell: Geology and. Nat. Hist. Survey of Minn., i, p. 541. R. D. Salisbury, Annual Rep't, N. J. Geol. Survey, for 1892, p. 155.

*Die Denudation in der Wüste und ihre geologische Bedeutung. Abhandlungen der math.-physisch. Classe der Königl. Sach. Gesell. der Wissenschaften. Band. xvi, No. iii, 1891, pp. 445–448.

The following are some of the foreign papers relating to the formation of carved pebbles:

"It was so far as I know on the 5th of April, 1876, that G. Berendt for the first time called the attention of North German geologists to peculiar facets which are seen on pebbles in the glacial drift. Since this time, these so-called 'drei-kanter, or 'Kantengerölle' have interested geologists, and a great deal has been written on this subject. They were regarded as the work of ice, of great pressure, etc., until Mickwitz in his treatise gave the right key to the puzzle, in that he proved that these facets are merely a phenomenon of smoothing by sand (Sandschlifferscheinung), a view which since then has merely been confirmed by Credner, Heim, Sauer, and others. Views at present differ only concerning the manner in which these planes were cut. I have collected a number of such edged pebbles in different parts of the desert: first I found them in Wady Araba in the Arabian desert, then between Gisch and Abu Roasch on the border of the Libyan desert; and Dr. Sarasin has found them in the Sinai peninsula.

"According to my observations," continues Walther, "it is limestone of the Cretaceous formation, of fine, compact, very uniform grain, which in Egypt forms dreikanter. I have not found other varieties of rock in this form. The Eocene limestones may be too soft, the Nummulitic rock too unequally hard, and the crystalline rocks, as I have to show in the following pages, so quickly decompose in the desert that they are not adapted for forming dreikanter.

"The number of corners ground on varies as much as does the size of the pebbles, and neither are causally connected. I found einkanter (pebbles with one edge or arris) a foot in diameter (fussgrosse) and nut-sized stones with five edges (fünfkanter) and conversely.

"The edges are of unequal sharpness, and I believe I have observed that the edges are merely due to the fact that two ground flat surfaces intersect each other so that the expression 'facetted pebbles' (Facettengerölle) appears to me to be more correct than edged-pebbles (Kantengerölle).

"A connection between the direction of the edges and the direction of the wind could not be found, and this seems to me easily understood since I did not observe constant winds, and

Johannes Walther: Kantengeröllen in der Galalawüste; Berichte der math. phys. Klasse der Sachs. Gesell. der Wissenschaft, Nov. 14, 1887; E. Geinitz: Ueber Kantengeröllen; Neues. Jahrb. für Min. Geol. and Pal. Stuttgart, 1887 (II), p. 78. Heim: Kantengeschiebe aus dem norddeutschen Diluvium; Vierteljahrsch r. d. Furcher. Natur. Gesellsch., 1888. F. Wahnschaffe, Beitrag. zur Lössfrage; Jahrbuch d. König Preuss. Geol. Landesanstalt, Berlin, 1890, pp. 328–346. A. Sauer und C. Chelim: Die Ersten Kantengeschiebe im Gebiete der Rheinebene; Neues Jahrb. f. Min., etc., 1890, ii. A. Sauer: Ueber due æolische Enstehung des Löss am Rande der norddeutschen Tiefebene; Zeitschr. für Naturwissenschaften, Bd. lxii, Halle, 1889; Neues. Jahrb. f. Min., 1888, Bd. ii, s. 300–304. Dames und Wahnschaffe: Zeitschrift d. Deutsch. Geol. Ges., 1887, s. 226–227 und 229. A. Geikie in the 3d edition of his text-book refers to other papers.

since the direction of the wind in the desert often changes every hour.

"On the other hand, I have never found the facetted pebbles isolated, but always distributed among ordinary desert pebbles, which lay close together. The annexed drawings show the position of facetted pebbles with reference to other pebbles.* I have frequently observed the movement of the sand between such pebbles with strong wind (sandwind) and have come to the following conclusion concerning the formation of the facettes:

"The sand flows along in little streams over the ground and the pebbles lying on the surface form just so many obstructions and resistances to the small sand-courses. In front of a large pebble the sand-stream is parted, so as often to reunite behind the obstruction; often the divided streamlets run, isolated, some distance further, finally reuniting with neighboring ones. In consequence of this bifurcation and reunion of the small sand-streams, caused by the stones lying on the ground, such stones upon which two converging sand-streams strike, become provided with two facettes, of which each has been formed by a sand-stream. Since these facettes continually enlarge, they come finally to an intersection (gegenseitigen Schneiden) and form thereby an edge. Pebbles which are constantly washed by similar sand-streams, receive sharp edges; but if the direction of the sand-stream changes, then the edges and facettes will become indistinct and again effaced.

"In other words, planes are cut on the pebbles, whose development forms edges, and on this account the word 'facetted-pebble' (Facettengerölle) it seems to me best expresses the process of their formation, for the edge is secondary."

The attention which is paid to this subject in Germany and the indications which point to even the slight action of wind-blown sands are evident from what Karl v. Fritsch says in his Allgemeine Geologie (Stuttgart, 1888), p. 302.

"Wir können in Norddeutschland die grossen erratischen Blöcke an den Wegen, Brückenfeilern, etc., nicht betrachten, ohne wahrzunehmen, dass sie fast alle auf der nach der herrschenden Windrichtung (meist Nordwest) liegenden Seite hin eine eigenthümlich flachgrubige oberfläche und einen stärkeren Glanz als liewärtz zeigen. Giebt es auch solche Feldsteine bei denen das erwähnte Verhältnisse nicht wahrnehmbar ist, so kann man die Ursache ihrer Rauhigkeit fast jedesmal erkennen. Sobald ein erratischer Block eine längere Reihe von Jahre unverändert liegen geblieben, fehlen diese spuren der Winderosion nicht."

Localities in which eolian action was observed in southern New England: Island of Martha's Vineyard.—At a number

*The drawings are not reproduced in the present paper.

of points on the highlands of this island wind-bared spaces were found, in some of which occur polished and worn pebbles. Such places are common on the east side of Menemsha Creek near the road which goes southward to Chilmark. At other points on the island, both in the highland region and on the morainal plain, I found carved and polished pebbles in positions where sands are not now blowing. One carved and polished pebble was found with its carved faces uppermost by the side of an old cart road in a field surrounded by woods, and in a location where no winds have blown sand since the foresting of that portion of the island. The facets still retain their peculiar polish. Another pebble partially buried in the soil had been covered over by a lichenous growth. It, too, had long been exempted from the action of the wind by the forest and soil. Near the mouth of Paint Mill Brook, on the north shore of the island, and at an elevation of about 20 feet above the sea, the glacial gravels underlying the soil, for the depth of 3 or 4 inches contain numerous examples of pebbles with a high polish and the facets and edges peculiar morainal plain near West Tisbury, all indicating the former to eolian pebbles. Similar pebbles were found on the activity of the process here described at a time before the growth of forests and the soil prevented the blowing of sand.

The section in Matakeset Creek.—The most important occurrence of facetted-pebbles known to me in this field was met with in September, 1889, in the extreme southeastern part of the island in the banks of a ditch opened between Matakeset Pond and Katama Bay, and which is known as Matakeset Creek. In this ditch, then newly opened, there was exposed to view a continuous line of sculptured and polished pebbles lying at an average depth of from one to two feet beneath the surface. The pebbly layer rested upon a semistratified deposit of sand and gravel constituting the marginal portion of the broad plain which fronts the Martha's Vineyard moraine on the south. The pebbly layer was overlaid by a deposit of fine, wind blown beach sand, stained black by humus, and surmounted by a thin layer of mould which nourished a growth of stunted beach grass. These sands are no longer blowing, but immediately to the south of the ditch at the distance of an eighth of a mile is a line of dunes fringing the shore, the inward migration of whose sands has not suffused the beach grass on the old wind-blown deposit. The layer of sand-carved pebbles is about three inches thick. The pebbles vary in size from the very smallest polished pebbles to bowlderets six inches in diameter. Many of these show no carving or polishing on one side, this being particularly the case with the underside where it was in contact with the underlying

gravels. This layer could be traced for a long distance either side of the point where I crossed the ditch, and thousands of characteristic facetted pebbles may be obtained from the layer. It was a noticeable fact that no facetted or polished pebbles occurred in the exposed faces of the underlying gravel section. The geological conditions of this section lend support to the idea that facetted pebbles are the product of eolian action. The location of this eolian pebble bed out on the southern margin of the frontal plain of the Martha's Vineyard moraine and on a surface which bears no marks of ice-advance excludes, it seems, the agency of strictly glacial causes from effecting the shape of these pebbles. They are evidently glacial stream pebbles reshaped *in situ*.

In the many instances on this island of soil-covered surfaces which have evidently undergone exposure to wind-blown sands, I am not certain of the time of the action, for it is obvious that since the process is now going on in places so it may have been active at any time in the past, the process being interfered with by the renewal of the vegetative coating. But the wide spread occurrence of buried or partially inhumed eolian pebbles, with worn sand, points strongly to the conclusion that the process has been more active than it now is.

Providence atlas sheet.—On the surface of the sand-plain in East Providence, R. I., is a bowlder 3 feet in length and 2 feet high, the southern face of which has been scored, worn and polished, evidently by the long continued action of blowing sand. The surface about the bowlder is now grass-covered. South of Providence in Auburn, bowlders exhibit traces of the same action. In many places on the west side of Narragansett Bay, the surface is covered by blown sand.

Boston atlas sheet.—Eolian pebbles occur in the soil near Fresh Pond in Cambridge. The pebbles of the Roxbury conglomerate, in a ledge, in Dorchester, exhibit the peculiar spoon-shaped and polished surface due to wind-erosion. Carved slates occur on the surface of kames in the soil along the Charles River in the vicinity of Mt. Auburn cemetery.

Boston Bay atlas sheet.—A bowlderet 6 inches in diameter bearing crateriform faces and possessing a high polish was found on Grover's cliff in Winthrop, the cobble having come evidently from the upper surface of the drumlin.

Narragansett Bay atlas sheet.—Eolian pebbles occur near the head of a glacial sand-plain half a mile north of the Warwick Neck Station in Warwick, R. I.

Dedham atlas sheet.—I found a pitted eolian pebble on the surface of a kame at Holbrook station on the Old Colony railroad.

Rate and Angle of Wear on Pebbles.—The observations made by W. P. Blake early led to the use of the artificial sand-blast in the arts. The conditions under which erosion takes place in the arts have been studied experimentally by Thoulet and are set forth in his paper, "Experiences synthetiques sue l'abrasion des roches par le sable."* The results of these experiments include observations relating to the rate of wear at certain angles of incidence which appear to me to be significant when compared with some of the pebbles which I am about to describe. "The abrasion," states Thoulet, "is so much the more energetic when the rock upon which it acts is more nearly vertical with reference to the direction of the sand which strikes (horizontally), and it diminishes very rapidly in intensity directly as the inclination becomes lower than 60 degrees." "Oblique incidence too (from 30° to 45°)† increases the rapidity of the cutting effect, probably because the issuing particles of sand are not met and their force deadened by the rebounding ones." From these experiments we should be led to expect the wear on a nearly vertical surface of a pebble to proceed slowly in nature, to go forward more rapidly after a time when a surface had been worn down to a plane inclined about 30° to the vertical, and again to wear very slowly when the plane had been worn down to an angle of 60 degrees with the vertical. The result of this varying rate of wear would be that on a pebble plain exposed to sand-blasting, the carved pebbles would soon pass through the maximum rate of cutting and would remain long near the angle of 30° of inclination to the horizon. An examination of several well-facetted pebbles from Martha's Vineyard shows this feature in a marked way. It is necessary, however, in order to ascertain this mature state of erosion, to establish the soil-line or horizontal plane in which the pebble lay at the time of its carving. This can frequently be done with pebbles found *in situ*. The smaller pebbles by the erosion of the sand and gravel about their bases have frequently been overturned, and eroded on all sides. It is evident that where inversion takes place regularly and frequently that facets may fail to develop and that the pebble may slowly waste away in the manner of those described by Gilbert on the plains of Colorado.

Lithological characters of the pebbles.—There is a certain relation between the sand blown upon the rocks and their

*Compt. Rend. Acad. Sci., t. civ, p. 381; also Annales des mines, Mars-Avril, 1887, pp. 199-224.

† The experiments conducted by Gen. Tilighman in connection with his patent are described by G. F. Barker in Johnson's New Universal Cyclopedia, vol. iv, pp. 62-65.

mineralogical constitution. The most perfectly symmetrical pebbles are a variety of compact quartzite, or even vein quartz. Finely crystallized granitic rocks also exhibit good facets, but the quartz is apt to stand out above the feldspar. Quartz-porphyries, felsites and other massive rocks exhibit forms related with original fragmental contour and mineralogic construction. Pits are reamed out where minerals have decomposed, and grooves are worn often regardless of structure but commonly along lines determined by weakness in the rock. Facets show no prevalent relations to joint-planes or bedding; but often one can see that a joint-face has been cut and polished.

The symmetry of the pebbles gives them an artificial appearance in many instances. The original symmetry of rounded water-worn pebbles has been preserved by the production of balanced planes on opposite sides of the fragment. Pyramidal, triangular pebbles are common. The smaller pebbles by reason of the facility with which they may be undermined and overturned by the wind, are more generally carved on all sides than the larger ones.

Nomenclature.—In Germany sand-blasted pebbles receive specific names depending upon the number of edges which are developed by the intersection of facettes. The term facetted-pebble proposed by Walther has already been given in this country to a product of glacial action.* Since some of the sand-carved pebbles are facetted, some grooved, and others merely pitted, I would suggest that "glyptolith" (*glyptos*, carved; *lithos*, stone) might be employed, in the sense of a rock surface carved by wind-blown sand. "Eolite" has already been appropriated for a rock made by the deposition of sand by æolian action.†

That the blowing of sand by wind must have been wide spread in the interval between the disappearance of the last ice-sheet and the occupation of the land by plants, is shown by the fact that even now wind-blown sand and sand-blasting is to be observed at numerous points on the sand-plains in this district. The question is to determine whether the glyptoliths found in grass-covered tracts or in wood-lands are due to a phase of general deflation preceding the incoming of the recent flora, and are, therefore, indicative of an eolian phase succeeding the deglaciation of the district, or whether they are due to secular wind erosion acting now and then as the opportunity is offered by the removal of the soil and the exposure of sand to the wind. My own observations have not been sufficiently extended to come to a definite conclusion in regard to this matter, particularly since in many places the

* Chamberlin: 7th Annual Report, U. S. G. S., p. 209, 1888.
† T. M. Reade: Geol. Mag., dec. ii, vol. ii, pp. 587, 588.

process of sand-carving is now evidently going on; but I am led by the distribution of many of the pebbles which I have collected to think that sand-blasting was formerly more prevalent than it now is in this district.

Relation of land to sea-level.—It is obvious that the carving and polishing of rock surfaces by the sand-blast is essentially a subærial phenomenon. Although the sand carried by water-currents as in the case of the Colorado, shown by Newberry, furrows and polishes the rock of its bed, the sands of the littoral are not known to produce the facets and polish characteristic of the typical glyptoliths. This is probably due to the frequent turning and rubbing of pebbles even where they lie in the zone of sands driven by wind along the beach. It follows that where we have these eolian pebbles carved at any time in the past, we have evidence to show that at that time the land was above the level of the sea. As yet the coast region of New England along which submergence at the close of the last glacial epoch has been suspected has not been examined with this point in mind.* The evident antiquity of the Matakeset examples on Martha's Vineyard and the direct superposition of the eolian pebbles on the wind-eroded surface of the overwash or frontal plain indicates that next after the deposition of the glacial gravels it was subjected to wind-erosion. This evidence indicates that if the land there was submerged after the retreat of the ice, the period of depression was brief and is locally without any trace of its geological effects.

Among the problems which these eolian pebbles may be looked to for a solution is the question of the former elevation of the continental shelf in recent geological times. It is therefore of the utmost importance that the pebbles and bowlders occasionally dredged up from the surface of the "banks" off the New England coast should be carefully scrutinized with the view of determining whether or not they bear marks of eolian erosion. If, as there is reason to believe, these banks were during late Tertiary or in the pleistocene period, like Martha's Vineyard and Nantucket now, above the sea-level, we should find on them the kind of evidence which occurs on the islands named.

Cambridge, Mass.

*Since this was written, Prof. W. M. Davis has presented in the current volume of the Proc. Boston Soc. Nat. Hist. a paper announcing the occurrence of facetted eolian pebbles at numerous points on Cape Cod in geological situations showing the subærial deposition of at least a part of the glacial gravels of that peninsula. See also paper by the same author read at the December meeting of the Geol. Soc. Am., held in Boston, 1893.

SCIENTIFIC INTELLIGENCE.

I. Chemistry and Physics.

1. *On the Production of Silver hyponitrite from Hydroxylamine.*—The fact that on mixing at ordinary temperatures hydroxylamine sulphate and sodium nitrite in concentrated aqueous solution, mutual decomposition takes place with the evolution of nitrogen monoxide, was first pointed out by V. Meyer. If the solutions be dilute, the same reaction takes place on heating; but if the temperature be not raised the decomposition proceeds slowly and if silver nitrate be added to this solution a yellow precipitate of silver hyponitrite is thrown down which may be purified by dissolving it in cold dilute nitric acid and reprecipitating with ammonia. W. Wislicenus has shown that this reaction may readily be used as a lecture experiment. For this purpose two or three grams of hydroxylamine sulphate and the equivalent quantity of sodium nitrite are dissolved separately in about 100^{cc} of water, the solutions are mixed, and a portion is tested with silver nitrate. A white precipitate of mixed silver sulphate and silver nitrite results. The rest of the solution is heated rapidly to 50° on the water bath for a few moments, the evolved gas being shown to be nitrogen monoxide by means of a glowing splinter. On adding now silver nitrate to the solution, a yellow precipitate of silver hyponitrite $Ag_2N_2O_2$ will be thrown down. Experiment will determine the time of heating necessary to secure the best results.—*Ber. Berl. Chem. Ges.*, xxvi, 771, April, 1893. G. F. B.

2. *On the Electrolysis of Alkali Salts.*—From recent experiments by Arrhenius, it appears that upon electrolyzing an aqueous solution of an alkali salt, using mercury as the cathode, a considerable time elapses after the passage of the current before hydrogen makes its appearance. From this fact it would appear that hydrogen is not a primary result of the electrolysis, but is due to the secondary action of the water of the solution upon the alkali amalgam produced primarily by the discharge of the positive ion; i. e. the alkali metal. The time required before the first bubble of hydrogen appears, increases very slowly as the strength of the current diminishes, and, when the current is maintained constant at one-twentieth of an ampere, increases as the concentration increases and as the temperature decreases. For equivalent solutions of electrolytes having the same positive ion, this time is practically constant; but it is found to be much larger for potassium salts than for those of sodium and lithium. Theory indicates that the electromotive force required for electrolysis increases, at the outset, with the amount of electrolytic products already separated. But when secondary reactions take place, preventing the continuous accumulation of these products if the electrolysis goes on very slowly, the electromotive force required

for electrolysis is determined almost solely by these secondary reactions. Evidently if the result of the secondary actions is the same as it is in the case of alkali salts, the electromotive force required for the decomposition will be practically the same also; thus confirming the experiments of Le Blanc and not requiring us to assume the primary decomposition of the water. From Shields' data, showing that a decinormal solution of sodium acetate is hydrolyzed to the extent of 0·008 per cent, the author calculates that the electrical conductivity of pure water at 25° is $0{\cdot}56 \times 10^{-11}$; this conductivity being greatly diminished in the presence of electrolysis. A salt of a strong acid and a strong base is hydrolyzed to the extent of $1{\cdot}21 \times 10^{-4}$ per cent in decinormal solution, the absolute quantity hydrolyzed being nearly independent of the concentration. The author has also calculated from the electromotive force required for the electrolytic dissociation of pure water, the value of this force required to develop hydrogen and hydroxyl ions from water in a liquid already containing as many of these as are in normal solutions of strong acids or bases. His value thus found is 0·81 volt, that obtained experimentally by Le Blanc being 0·76 volt.—*Zeitschr. physikal. Chem.*, xi, 805, June, 1893. G. F. B.

3. *On the Preparation of Chromium, Manganese and Uranium in the Electric Furnace.*—The electric furnace of MOISSAN and VIOLLE consists of a carbon vessel in which an arc from two horizontal electrodes is made to act upon any substance desired. This vessel is a portion of a carbon tube, of a height equal to its diameter, standing on a carbon plate and covered with a disk of the same material. The electrodes are also of carbon; all the carbon parts of the apparatus being prepared from powdered gas carbon and tar, no boric acid being used. The carbon cylinder and electrodes are enclosed in a block of lime, being supported on props of magnesia and separated from the sides of the block by a layer of air five millimeters thick. The dimensions of the furnace and its various parts vary according to the current which is to be used and the temperature desired; a temperature of 3000° being obtained without difficulty. If manganous oxide be mixed with charcoal and heated by the arc given by a current of 300 amperes and 60 volts, the reduction is complete in five or six minutes; but twelve to fifteen minutes is necessary if only 100 amperes and 50 volts is used. If the carbon be in excess, the reduced metal contains 6·4 to 14·6 per cent of this substance, but if the manganous oxide be in excess, only 4 or 5 per cent of carbon is contained in the metal. With a low content of carbon, the metal is unaltered when exposed in open vessels; but when the proportion of carbon is increased, it is attacked by moist air. In water, small fragments are oxidized, hydrogen and hydrocarbons being evolved. Chromium oxide mixed with carbon and subjected to a current of 350 amperes at 50 volts, is reduced in from 8 to 10 minutes, 100 grams of the metal being obtainable at one operation. With 100 amperes and 50 volts, fifteen min-

utes are required for the reduction. The metal contains from 8·6 to 12·85 per cent of carbon; but if placed in fragments in a carbon crucible brasqued with chromium oxide and again subjected to the arc, the metal is obtained quite free from carbon. If chromite be thus treated an alloy of chromium is obtained. If the mixture of oxides obtained by calcining uranium nitrate be mixed with a slight excess of carbon, strongly compressed in a crucible of carbon, imbedded in magnesia, and submitted to the arc produced by a current of 450 amperes at 60 volts, reduction takes place at once, an ingot of 200 to 220 grams being obtained in 12 minutes. The metal is very hard, has a brilliant fracture, and contains from 5 to 13·5 per cent of carbon. Although its fusing point is higher than that of platinum, it decomposes water at the ordinary temperature.—*C. R.*, cxvi, 347, 349, 549, Feb., March, 1893. G. F. B.

4. *On the Production of Sarcolactic acid by the Fermentation of Inactive Lactic acid.*—Having obtained a bacterial growth possessing the power of exciting an active fermentation in solutions of calcium lactate, FRANKLAND and MACGREGOR have studied the resulting products. They find that by the interrupted bacterial formation of ordinary inactive calcium lactate a liquid may be obtained which shows a negative rotation in the polarimeter. From this liquid they have extracted the residual lactic acid and by converting it into the zinc salt have obtained by fractional crystallization a lævorotatory zinc lactate proved by its chemical composition and its specific rotation to be pure zinc sarcolactate. By converting this into the calcium salt, it was found to possess the same specific rotation as calcium lactate. If the fermentation was interrupted too early a large quantity of the inactive acid remained mixed with the active one; while if too long continued, the active lactate was destroyed.—*J. Chem. Soc.*, lxiii, 1028, Aug. 1893. G. F. B.

5. *On the Heats of Combustion of Gaseous Hydrocarbons.*—Carefully purified gaseous hydrocarbons have been burned by BERTHELOT and MATIGNON in a calorimetrical bomb by means of compressed oxygen and their heats of combustion in this way determined. Preliminary experiments made with hydrogen gave 68·15 large calories at constant volume and 68·99 large calories at constant pressure; the values obtained for carbon monoxide being 67·9 and 68·2 respectively. This gives +26·1 for the heat of formation of this latter substance. For the hydrocarbons, the values obtained were as follows:—

		Methane.	Acetylene.	Ethylene.	Ethane.
Heat of combustion	Const. vol.	----	----	340·05	370·9
	Const. press.	213·5	315·7	341·1	372·3
Heat of formation		+18·7	+58·1	−14·6	+23.3

		Propane.	Allylene.	Propylene.	Trimethylene.
Heat of combustion	Const. vol.	526·7	472·4	497·9	505·6
	Const. press.	528·4	473·6	499·3	507·0
Heat of formation		+30·5	−52·6	−9·4	−17·1

It will be observed that the difference between the heats of combustion of two homologous and contiguous hydrocarbons is practically constant, being about 157 Calories; while the corresponding difference between the heats of formation is about 5·5 Calories. In the methane series therefore the heats of combustion at constant pressure are $213{\cdot}5+157{\cdot}5n$, in the ethylene series $341{\cdot}2+157.5n$ and in the acetylene series $315{\cdot}5+157{\cdot}5n$; the corresponding heats of formation being respectively $+18{\cdot}7+5{\cdot}7n$, $-14{\cdot}6+5{\cdot}5n$, and $-58{\cdot}1+5{\cdot}5n$. When a member of the acetylene series is converted into the corresponding olefine, 43·3 Calories is set reef; and the conversion of the olefine into the paraffin develops 39 Calories. The heat of combustion of trimethylene does not accord with the hypothesis that it is a closed chain hydrocarbon. The heat of formation of trimethylene dichloride, 4·3 Calories, shows that the substitution of Cl_2 for H_2 produces a thermal disturbance similar to that observed in the methane series.—*C. R.*, cxvi, 1333; *J. Chem. Soc.*, lxiv, ii, 444, Oct. 1893. G. F. B.

6. *On the Action of a Lævolactic Ferment on Dextrose, Rhamnose and Mannitol.*—In the course of observations on the nature of the micro-organisms which attack the ripe pear, TATE succeeded in isolating one which produced marked lævolactic fermentation of dextrose and mannitol. He has now studied this fermentation and gives the following conclusions: The organism which produces this lævolactic fermentation is anaërobic and is characterized by forming two kinds of growth upon solid media —a white growth of moist appearance consisting chiefly of rods and cocci, and a tough, tapioca-like growth consisting of ascococci. The fermentations brought about by this organism under aërobic conditions in solutions of dextrose, mannitol and rhamnose are fairly constant in character. From 9 molecules of dextrose the organism produces alcohol 2 molecules, succinic acid 1, lævolactic acid 7 to 8; and acetic and formic acids in smaller and variable proportions. If the organism has grown in the ascoid form, the amount of lævolactic acid is slightly less probably from secondary changes. From 9 molecules of mannitol are obtained 6 of alcohol, 1 of acetic acid, 2 of formic acid and 12 of lævolactic acid, together with some succinic acid. From 9 molecules of rhamnose, 4 molecules of *inactive* lactic acid and 5 of acetic acid are produced, alcohol being absent. This cultivation in rhamnose solutions, producing inactive lactic acid, does not cause the organism to use any of its activity in causing lævolactic fermentation of dextrose. The study of dextrose fermentations, in the opinion of the author, tends to indicate that the attack of the organism is directed simultaneously against nine or other multiple number of molecules—that is against a group of molecules—rather than against single molecules.—*J. Chem. Soc.*, lxiii, 1263, Oct. 1893. G. F. B.

7. *On Lead Tetracetate.*—It has long been known that red lead dissolves in hot glacial acetic acid yielding a colorless solution which on cooling deposits crystals. HUTCHINSON and POL-

LARD have examined these crystals with considerable care. They were purified by recrystallization from hot glacial acetic acid and were dried over sulphuric acid. When free from acetic acid the substance is extraordinarily sensitive to the presence of water, which at once converts it into lead peroxide and acetic acid. This fact was made use of to analyze the substance, the lead peroxide being collected on a tared filter and the acetic acid titrated with alkali. The results leave no doubt that the compound is lead tetracetate $Pb(C_2H_3O_2)_4$. The crystals begin to melt at 175° and decompose a few degrees above this. They dissolve in concentrated hydrogen chloride, producing lead tetrachloride; as is shown by the fact that when a cold saturated solution of ammonium chloride is added, a characteristic yellow double ammonium salt is precipitated. An analogous crystalline tetrapropionate may be similarly obtained.—*J. Chem. Soc.*, lxiii, 1136, Sept. 1893.

G. F. B.

8. *On the Thermochemistry of Chloracetic acid.*—It has been observed by TANATAR that the unstable modification of chloracetic acid (fusing point 52°) changes into the stable modification (fusing point 63°) with an evolution of heat of 0·65 calory per gram-molecule. The heat of dissolution of the unstable acid was found to be −2·77 calories, that of the stable acid −3·47 calories; the difference being 0·70 calory, nearly the same as the heat of transformation directly observed. Both acids show a normal molecule in aqueous solution and a double molecule in solution in benzene.—*J. Russ. Chem. Soc.*, xxiv, 694; *J. Chem. Soc.*, lxiv, i, 624, Nov. 1893.

G. F. B.

9. *A method of using the arc light for reading rooms and libraries.*—Mr. Benjamin A. Dobson, a manufacturer at Bolton, England, has lately made experiments in regard to the best method of lighting his workshops which are very suggestive in regard to the disposition of lights in reading rooms and libraries. It was found that incandescent lamps were not much superior to gas while the arc lamp threw sharply defined shadows. It therefore occurred to Mr. Dobson to invert the ordinary arc lamp, so that the positive carbon from which most of the light proceeds was below the negative carbon. The light which was thus thrown upward by the positive carbon was received on a well whitewashed ceiling and was thus reflected downward and the room was flooded with a well diffused light. One could stand in any part of the rooms thus lighted and read a book or paper without being troubled with shadows. When the strength of the light is considered the system is cheaper than that of gas.—*Nature*, Nov. 2, 1893, p. 18.

J. T.

10. *Application of light waves to metrology.*—Michelson in a preliminary communication states that two complete and independent determinations give for the numbers of waves of red light in one meter of air at 15° C. and 76^{mm} pressure the following:

1st series	1 5 5 3 1 6 3 . 6
2d series	1 5 5 3 1 6 4 . 6

The difference from the mean is half a wave, or about one-fourth of a micron. We can thus compare two standards by means of light waves with the same order of accuracy that we can at present compare the meter bars, and the comparison is based upon an unchangeable unit.—*Nature*, Nov. 16, 1893. J. T.

11. *Radiation of heated gases.*—F. Paschen, by means of a bolometer, has conducted a series of experiments upon the radiation of heated air, oxygen, carbonic acid and steam. The gases were heated by passing over a platinum spiral which was raised to incandescence by a storage battery. A heat spectrum was formed and wave-lengths as long as $5{\cdot}2\mu$ were measured. It was found that gases can emit a discontinuous spectrum. In the case of CO_2 and steam, there was a displacement of the intensity within the emission maxima, with decreasing temperatures. No explanation is given of this phenomenon.—*Ann. der Physik und Chemie*, No. 11, 1893, pp. 409–443. J. T.

12. *Electric radiations in Copper Filings.*—At a meeting of the Physical Society, London, Oct. 27, Mr. W. B. Craft stated that if a battery-galvanometer and tube containing copper filings were joined in series under ordinary conditions no current passed; but a current was immediately produced by an electric spark, produced many feet distant by an electrical machine. Iron filings were inferior to copper, and carbon always allowed the current from the battery to pass.—*Nature*, Nov. 9, 1893, p. 46. J. T.

13. *Absorption of Electrical waves.*—I. Klemenčič has applied his method of the thermal junction to the question of the absorption of the energy of electrical oscillation on wires of different material and confirms the results of Bjerknes, J. J. Thomson and Trowbridge in regard to the damping effect of iron. For wires made of iron, german silver, brass and copper of 6^{cm} long and $0{\cdot}018^{cm}$ diameter he finds the development of heat can be expressed relatively as $10{\cdot}5 : 1{\cdot}75 : 1 : 1$. The observations showed that in the case of the ramification of electrical waves of very short duration the factor of self-induction is more potent than that of resistance.—*Ann. der Physik and Chemie*, No. 11, 1893, pp. 456–475. J. T.

14. *A new form of Contact maker.*—In a paper read before a late meeting of the American Institute of Electrical Engineers, Messrs. Bedell, Miller and Wagner describe a new form of contact maker. A fine jet of water was projected upon a needle which is inserted into a revolving disc, and thus contact was made between a voltmeter and a circuit. The nozzle of the water jet is carried by a disc which is capable of being rotated and has its edge graduated into degrees. The voltmeter could thus be connected with the circuit of a transformer at any required part of a cycle. The needle cut the water jet before it broke into drops. A little salt was put into the water, for conduction. J. T.

II. Geology and Mineralogy.

1. *Geological Survey of Georgia.* The Paleozoic group. The Geology of ten counties of northwestern Georgia and resources; by J. W. Spencer, state geologist, pp. 406 and one map, Atlanta, Ga. 1893.—The Paleozoic formations of northwestern Georgia are described under the following nomenclature, viz: Cambrian, Oostanaula formation; Ordovician, Knox dolomite, Chicamauga (including Rockmart slate); Silurian, Redmountain; Devonian, Chattanooga Black shales; Carboniferous, Fort Payne chert, Floyd shales, Mountain limestone and Coal measures. Part II describes the Economic Resources of the Paleozoic group, coal, iron, manganese, bauxite, kaolin and building materials, graphite, etc. The report is a valuable compendium of the local geology of this corner of the state. W.

2. *The Geology of Mexico.*—The following maps and charts illustrating the geology of Mexico have been published recently by the *Comision geologico Mexicana*, prepared under the direction of Antonio del Costillo, director of the Escuela n. de Ingenieros and of the Comision geologico:—A small geological map of the whole Republic on the scale of 1:10,000,000, entitled Bosquejo de una Carta geologica de Republic Mexicana; a map on a scale of 1: 2,000,000, on which the distribution of the following minerals is shown, viz: gold, platinum, mercury, copper, iron, lead, bismuth, zinc, antimony, coal, sulphur, kaolin, lignite. Carta los meteritos de Mexico, scale 1: 10,000,000. Cartes geologicas de Pozos Artesianos abiertos en la gran Cuenca de Mexico. Plans of the Peñon de los Baños, scale $\frac{1}{1000}$: of the mining regions of S. Antonio and Triumfo in lower California, scale $0{\cdot}^{m}001$ per 40^{ms};—of the iron mines of Encarnacion and of S. Jose del Oro, scale 1: 20000, and a detailed plan of the geological and petrographical structure of the environs of the city of Mexico, scale 1: 200,000. In the same series appears a large engraving of the geyser in the vicinity of Puebla described by Antonio del Castillo.

A valuable summary of the present state of knowledge regarding the geological formations and their fossils for Mexico is given in *Datos para la Geologia de Mexico*, by Jose G. Aquilera and Ezequiel Ordoñez of the Escuela de Ingenieros, Mexico, 8vo, 87 pp. The following systems are recognized: Archæan, Permo-Carboniferous, a small outcrop near the border of Guatemala, compact limestone with a *Productus? semireticulatus*, and certain tracts in the northeastern part of Mexico, which furnish fossil Lamellibranchs, which may be Carboniferous. Triassic, in numerous localities, determined to be upper Triassic. From this system in Ros Bronco in Sonora, Dr. Newberry described a flora of 23 species of plants. The Jurassic system is recognized by a fauna of 37 species distributed among the Sponges, Echinoderms, Vermes, Brachiopods, Lamellibranchiata and Cephalopoda.

Among the latter are *Arietites* indicating the inferior Lias and *Stephanoceras* indicating a middle Jurassic horizon.

The Cretaceous system is indicated by a rich fauna including species of lower, middle and upper Cretaceous age. This fauna includes 3 species of Rhizopoda, 38 Anthozoa, 16 Echinoidea, 2 Vermes, 95 Lamellibranchiata, 54 Gastropoda, 24 Cephalopoda, 1 Thoracostraca, 8 Pisces, 1 Reptile, a total of 242 species.

The Tertiary system is recognized by 67 species of ? Eocene and Miocene age, including 3 Echinodermata, 53 Lamellibranchiata and 11 Gastropoda.

Hippotherium peninsulatum Cope and *Protohippus Costilloi* Cope, are from the Upper Miocene, and from the Pliocene and Quaternary are the following genera of vertebrates; *Spheroma?*, *Glyptodon*, *Sceledoterium*, *Rhinoceros?*, *Equus*, 5 species, *Platigonus*, 2 species, *Palauchenia*, *Auchenia*, 2 species, *Holomeniscus*, *Eschatius*, *Bison*, *Aphelops*, *Mastodon*, (1 *Trilophodon*, 2 *Tetralophodon* species), *Elephas*, 2 species. W.

3. *Geological Survey of New Jersey.* (Ann. Rept. 1892. 367 pp. 8vo and two folded maps.)—This report includes Pt. I, Surface Geology by Rollin D. Salisbury; Pt. II, Preliminary Report on the Cretaceous and Tertiary, W. B. Clark, with a geological map of portions of Monmouth and Middlesex Counties, scale 1 mile to the inch. Lists of the invertebrate fossils are given for the Lower Marl, Middle Marl, Upper Marl, and Shark River Marl beds, and Heilprin's list of the Neocene fossils. A chapter is devoted to discussing the origin of Greensand, and three plates of colored illustrations of the mode of formation of Glauconite grains are reproduced from the Challenger Report on Deep Sea Deposits. Part III, is on the Water supply and Water power by C. C. Vermeule and is accompanied by a map of the state. Part IV, Artesian wells of southern New Jersey by Lewis Woolman. V. Notes on Sea Dikes of the Netherlands and Reclamation of the Lowlands of the Netherlands by the state geologist. W.

4. *Elementary Paleontology for geological students*; by Henry Woods, B.A., F.G.S. pp. 222, small 8vo, 1893. (Cambridge University Press, London).—This little hand-book places before the British geologist a concise account of the chief characters of the hard parts of invertebrates, sufficient for the recognition and the classification of fossils into their proper class, order and sub-order, and for some of the more important genera brief diagnoses of their characters are given and the geological range of each. Except that the subdivision of the geological systems are in terms of British geology, the book will be a useful one to the American geologist who is chiefly interested in identifying the age of rocks by their fossils.

5. *Geology of Boston Basin;* by Wm. O. Crosby. Vol. i, Part I, Nantasket and Cohasset, 177 pp., 8vo. (Occasional Papers Boston Society of Natural History, IV).—This is a contribution to the detailed structural geology of Eastern Massachusetts illustrated by maps and numerous figures and sections. A chapter is included on the microscopic examination of the newer eruptive Rocks of Nantasket by Geo. P. Merrill. W.

III. Miscellaneous Scientific Intelligence.

1. *The Wilder quarter-century Book.*—A collection of original papers dedicated to Prof. B. G. Wilder at the close of his 25th year of service in Cornell University by some of his former students (D. S. Jordon, Mrs. A. B. Comstock, J. H. Comstock, E. R. Corson, L. O. Howard, Theob. Smith, W. C. Krauss, Mrs. S. P. Gage, H. M. Biggs, P. C. Branner, Y. A. Moore, G. S. Hopkins, P. A. Fish, W. R. Dudley, S. H. Gage). 8vo, pp. 493, 1893.

The articles are original investigations, in many cases beautifully illustrated, in Anatomy, Histology, Bacteriology, Pathology, Embryology, Botany, and Geology; some of them studies of problems of Evolution. Prof. J. C. Branner (Leland Stanford University), contributes valuable observations upon an Erosion in the hydrographic basin of the Arkansas River above Little Rock, Ark. From observations extending from Oct. 1887 to Sept. 1888, it is estimated that the amount of material carried past Little Rock by the river in one year is 28,299,929 tons, not including the mud and sand pushed along the bottom. At this rate of transportation it is estimated that the Arkansas River would remove an average of one foot from its total hydrographic basin in 9433 years.

Prof. J. H. Comstock (Cornell University) contributes a suggestive article on Evolution and Taxonomy, in which a minute comparison of the venation of the wings of insects is made the basis of their classification and of determining their genetic relations and evolution.

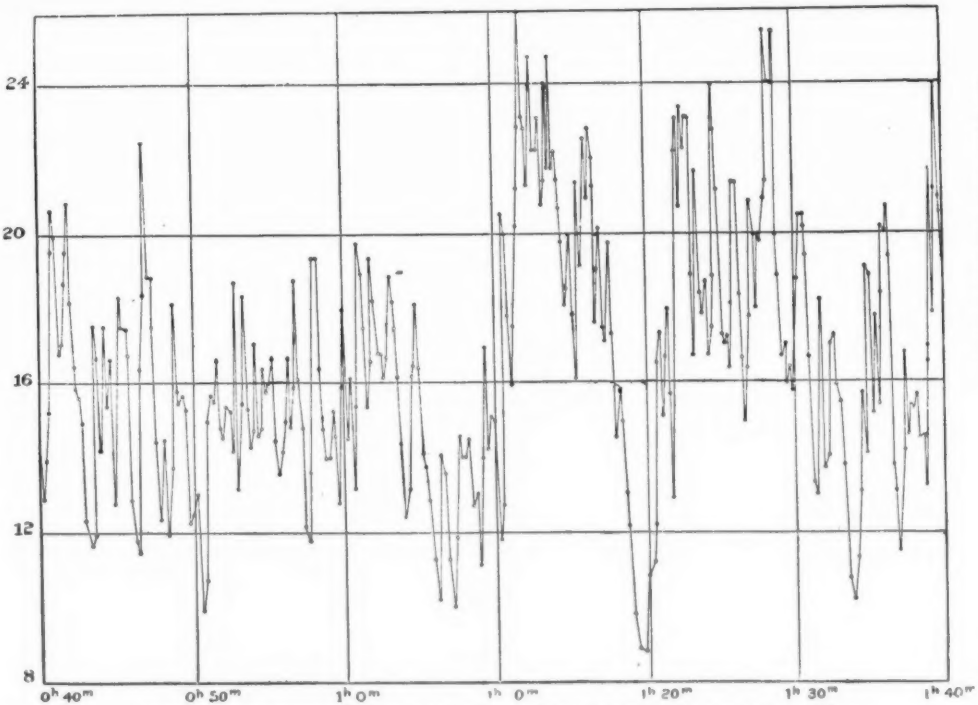
Prof. S. H. Gage (Cornell University) discusses the habits and structure of the Lampreys of the fresh water lakes of New York, assumes that the lake Lamprey is a landlocked species and recent offshoot from the true anadromous sea Lamprey. In order to account for this mode of origin the hypothesis is advanced that the ancestors of the Cayuga lake Lamprey were spawn deposited by the sea Lamprey which ascended the Susquehanna River at the geological time when Cayuga Lake emptied through it to the Atlantic; that they were landlocked by the opening of a lower exit for the waters through the Mohawk and retreat of the glacier northward, and then became adjusted to completely fresh water habitat. W.

Obituary.

Dionys Stur, the veteran geologist and paleontologist of Vienna, and late director of the K.K. geologische Reischsanstalt of Austria, died at Vienna October 9th, 1893.

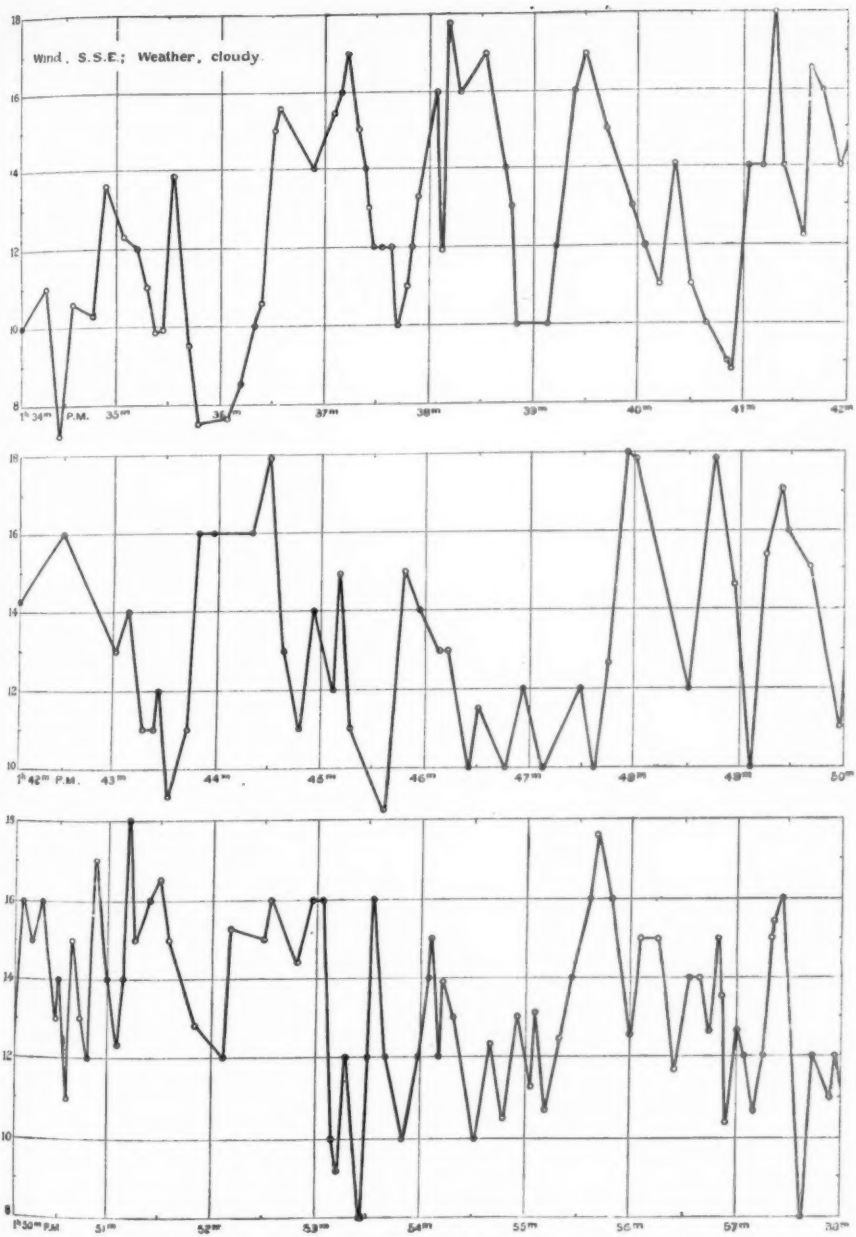
John Tyndall, the famous physicist, whose lucid and graceful style has made the reading public familiar with many of the abstruse problems of modern science, died at Haslemere, England, on December 4th, 1893, age 73 years.

Erratum.—On page 54, line 14, read *in* for *to*.



Wind velocities recorded July 16, 1887, at the Allegheny Observatory, with a Robinson anemometer registering every 25 revolutions.

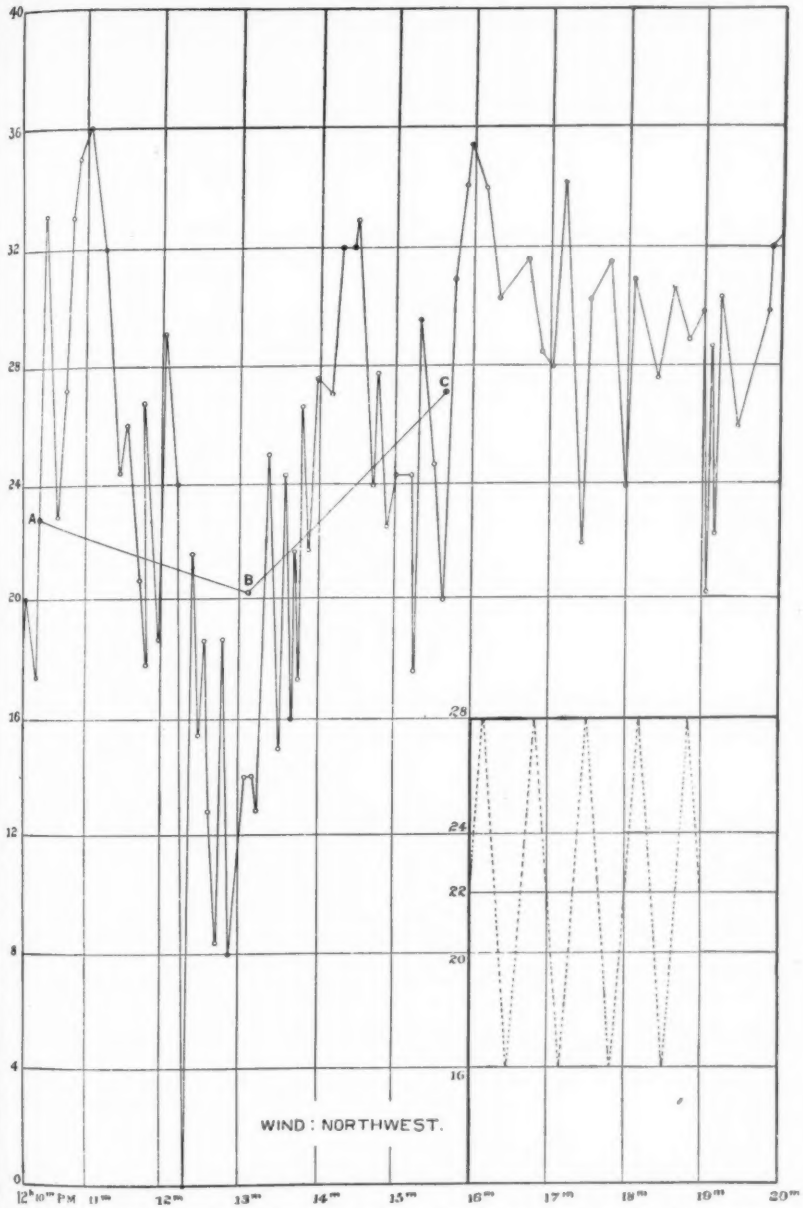
Abscissae = Time.
Ordinates = Wind velocities in miles per hour.



Wind velocities recorded January 14, 1893, at the Smithsonian Institution, with a light Robinson anemometer (paper cups) registering every revolution.

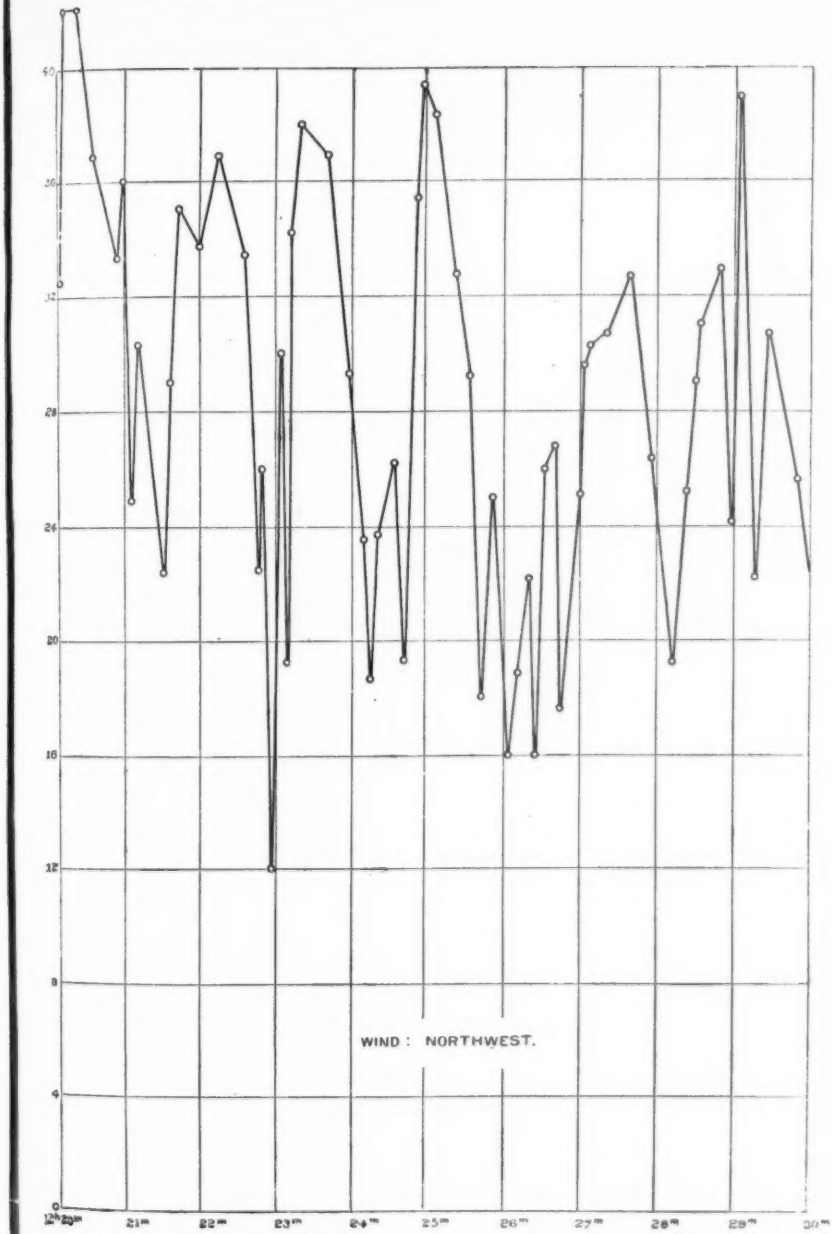
Abscissae = Time.

Ordinates = Wind velocities in miles per hour.



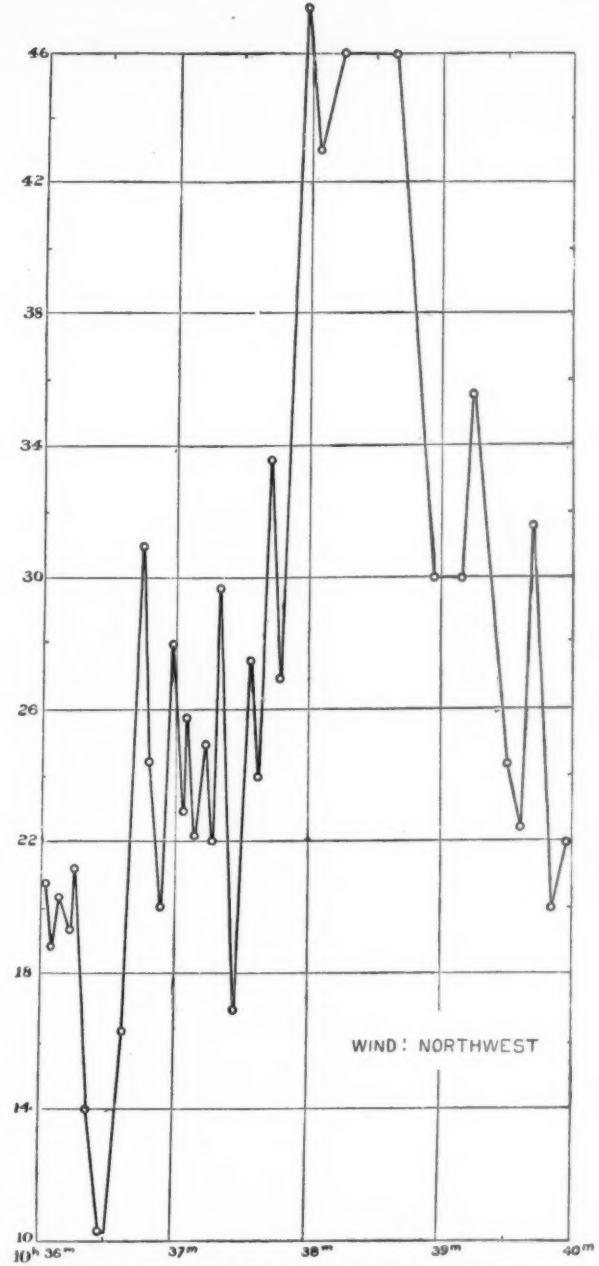
Wind velocities recorded February 4, 1893, at the Smithsonian Institution, with a light Robinson anemometer (paper cups) registering every revolution.

Abscissae = Time.
Ordinates = Wind velocities in miles per hour.



Wind velocities recorded February 4, 1893, at the Smithsonian Institution, with a light Robinson anemometer (paper cups) registering every revolution.

Abscissae = Time.
Ordinates = Wind velocities in miles per hour.



Wind velocities observed at Smithsonian Institution, February 20, 1893, with a Robinson anemometer (aluminum cups) registering every five revolutions.

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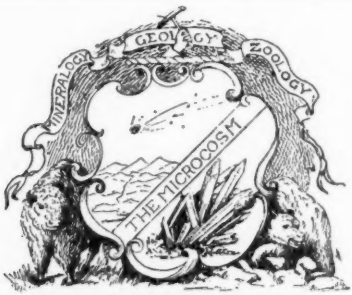

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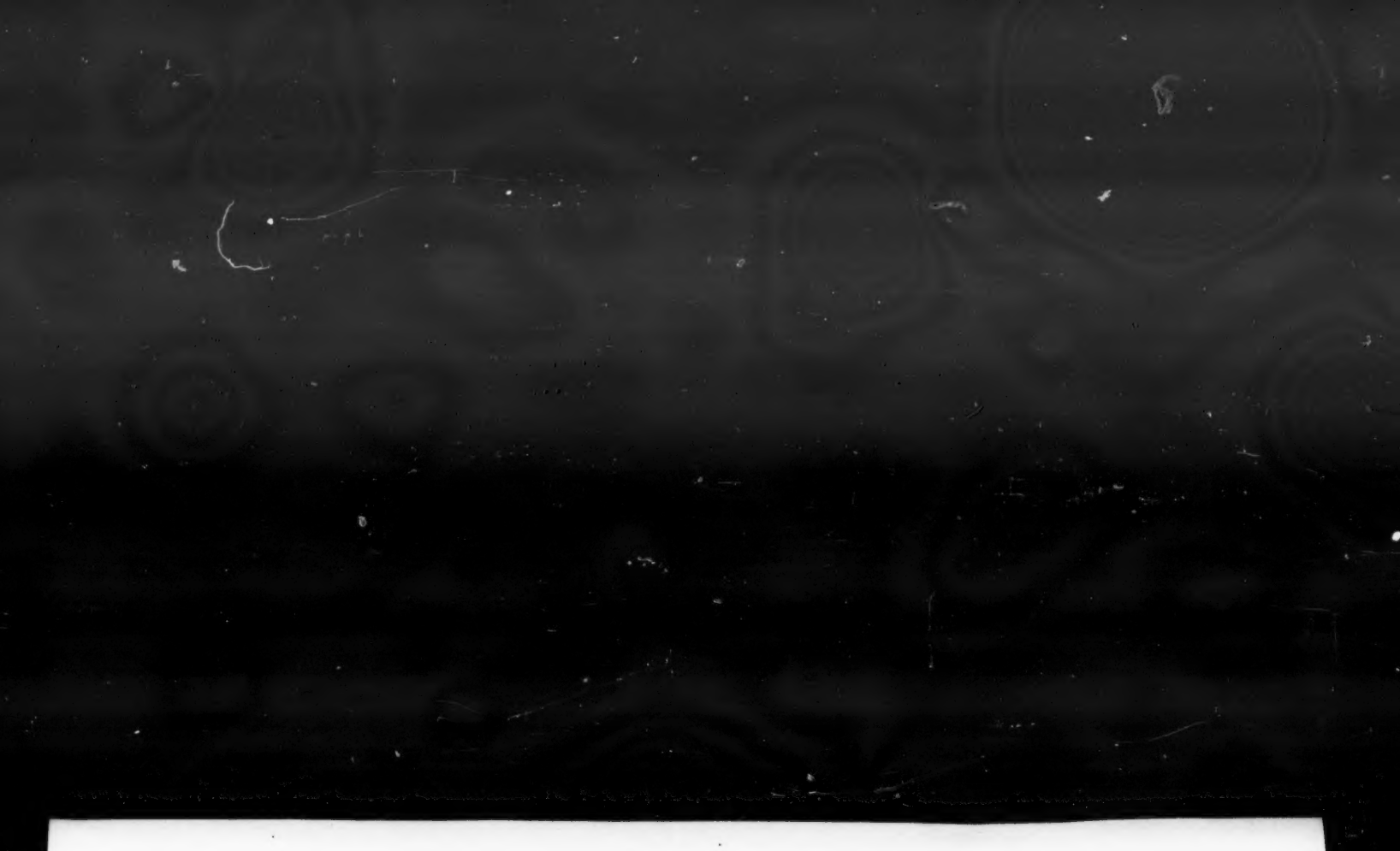

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